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ERRATA.

P. 186, 3d line from top, for "type are" read "type, are."

P. 187, 5th line from bottom, for "effected" read "affected."

P. 188, 12th line from bottom, after "Adolph Mayer" the sentence should be continuous; thus "Adolph Mayer, I find, etc."

P. 191, 7th line from top, for "clay, permeating" read "clay, but permeating."

P. 191, for "differing so in" read "differing in."

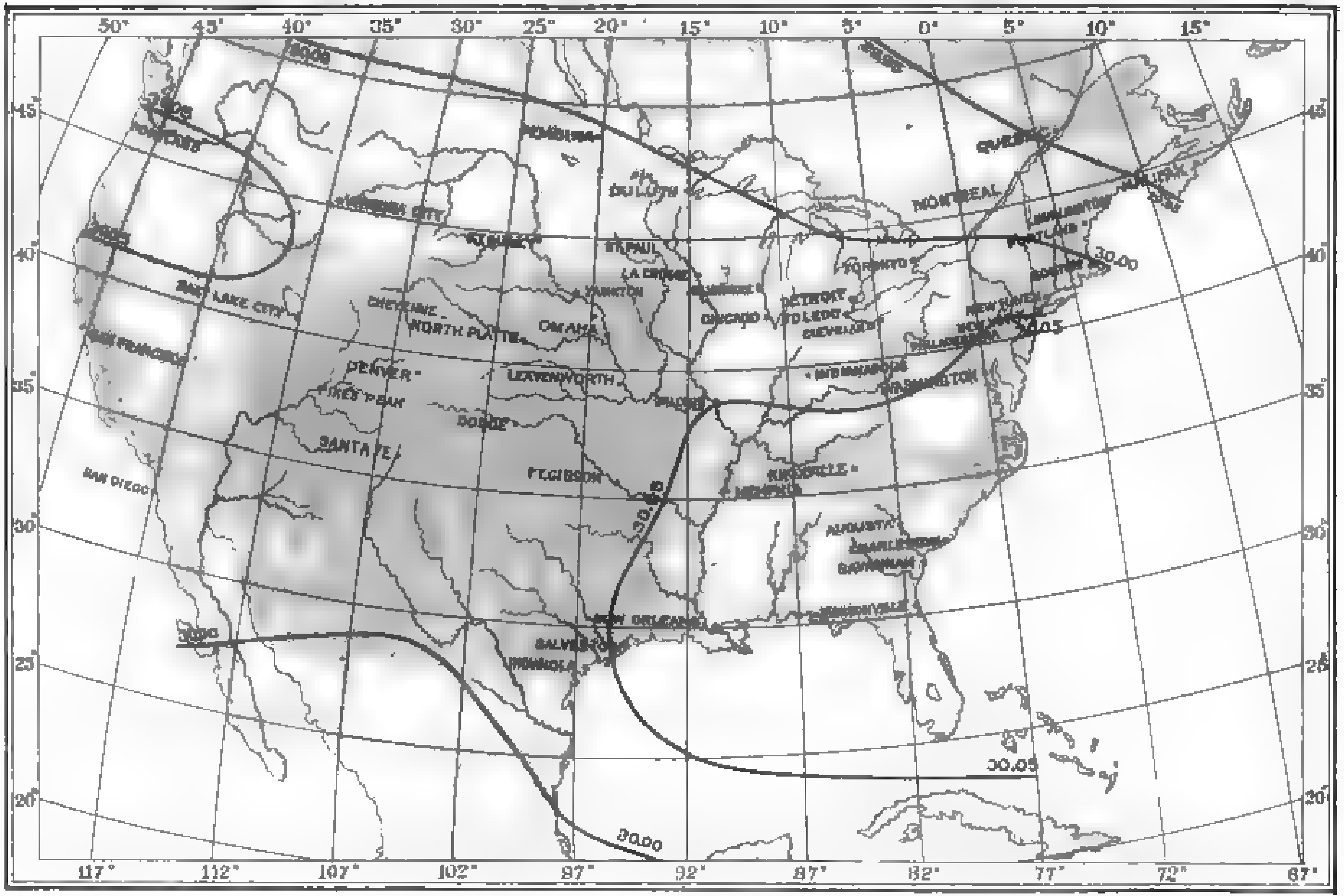
P. 192, 11th line from top, for "proportionately" read "proportionality."

P. 240, 4th line from bottom, for Capt. W. H. Dow, read Prof. W. H. Dall.

P. 286, 19th line from bottom, for "Prototheria" read "Pantotheria."

122-1881

MEAN PRESSURE OF THE ATMOSPHERE FOR THE YEAR. PLATE 1



THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. I.—*Contributions to Meteorology: being results derived from an examination of the observations of the United States Signal Service, and from other sources;* by ELIAS LOOMIS, Professor of Natural Philosophy in Yale College. Fifteenth paper, with Plate I.

[Read before the National Academy of Sciences, Washington, April 19, 1881.]

Reduction to sea-level of barometric observations made at elevated stations.

DURING the past eight years a large portion of my time has been devoted to investigating the course of storms in their progress across the Rocky Mountains, and in my first paper a storm was traced from Portland, Oregon, eastward to Lake Superior. During these eight years, I have had the constant services of a paid assistant, who has expended a vast amount of labor in attempting to discover the best method of tracing storms across the mountains. Some of the results of these investigations have been communicated in preceding papers, particularly Nos. 8, 9 and 13.

In order to study this subject more thoroughly, I have made a careful examination of the reduction to sea-level of the barometric observations made on Mt. Washington. I first prepared a table showing the reduction to sea-level, according to Dunwoody's Tables (S. S. Report for 1876, p. 354), for the entire

range of temperature and pressure experienced on Mt. Washington. I next computed the reduction according to the formula of Laplace, as developed in Guyot's Tables published by the Smithsonian Institution (Guyot's Meteorological Tables, series D, page 33), taking account of all the minute corrections. I next computed the reduction according to the formula of Plantamour, as developed in the Tables of Colonel Williamson (Professional Papers of the Corps of Engineers, No. 15). In order to compare these Tables with the actual observations, I took the monthly averages for Mt. Washington, as published in the Annual Reports of the S. S. for eight years (1872–1879); subtracted 6·36 inches for each month, and the remainder was regarded as the mean observed height. I took the mean between the reduced heights at Burlington, Vt. and Portland, Me., and used the result as representing the height of the barometer at sea-level under Mt. Washington. The difference between this result and the preceding gives the observed reduction of the Mt. Washington observations to sea-level. The mean of the temperatures at Burlington and Portland was taken to represent the temperature at the base of Mt. Washington, and the mean between the temperatures at the summit and base was regarded as the mean temperature of the column of air extending from the summit of the mountain to sea-level. When several months of the eight years observations gave about the same temperature and pressure, they were combined in a single mean. I thus obtained thirty values of the reduction from summit to sea-level, for a considerable range of temperature and pressure.

In order to extend the comparison to the greatest possible range of temperature and pressure, I selected the following list of dates from the published volumes of the tri-daily observations, now embracing a period of thirty-six months. 1. All the dates on which the thermometer on Mt. Washington fell ten degrees below zero, and also all the dates on which the thermometer at Burlington or Portland fell to ten degrees above zero. 2. All the dates on which the thermometer on Mt. Washington rose as high as 55°, and all the dates on which the thermometer at Burlington or Portland rose to 80°. 3. All the dates on which the barometer on Mt. Washington or at Burlington or Portland sunk 0·40 inch below its normal height; 4. All the dates on which the barometer at either of the stations rose 0·30 inch above its normal height. These four classes together embraced 423 days. For each of these dates, the mean pressure on Mt. Washington (from the three daily observations) was determined; the mean pressure at Burlington and Portland, and also the mean temperature at Burlington and Portland. These results enabled me to extend the observed reduc-

tion to sea-level from the barometric height 22·7 inches to 24·2 inches and from the temperature −10° to +65°. In order to smooth down the inequalities of the observed numbers, I took the mean between each three consecutive numbers corresponding to the same temperature, and substituted this result for the middle number. It is presumed that the results thus obtained represent pretty nearly the results which would be obtained from observations extending over a long term of years.

The results thus described are exhibited in the following Table, in which the height of the barometer on Mt. Washington, from 22·7 to 24·2 inches is given at the top of the table, and the mean temperature of the air column from −10° to +65° is given on the left margin. Corresponding to each temperature given in the table are four horizontal lines, the first of which (marked D), gives the reduction to sea-level as computed from Dunwoody's Tables; the second horizontal line (marked L), shows the reduction computed from Guyot's Tables founded on the formula of Laplace; the third horizontal line (marked P), shows the reduction computed from Williamson's Tables, which are based on those of Plantamour; the fourth horizontal line (marked O), shows the reduction deduced from actual observations as above described.*

An examination of this table shows the following results:
1. Dunwoody's Tables accord very well with those derived from the formula of Laplace, the differences ranging from +0·011 inch to −0·041 inch.

2. The differences between the formulas of Laplace and Plantamour range from +0·030 inch to +0·103 inch, the reduction by Laplace being on an average 0·053 inch greater than by Plantamour.

3. The reductions deduced from the actual observations differ very much from either of the values above computed; the differences from Laplace ranging from +0·263 to −0·105 inch. These differences follow a remarkable law. According to the formula of Laplace, when the pressure on Mt. Washington increases from twenty-three inches to twenty-four inches without any change of temperature, the reduction to sea-level is increased by $\frac{1}{23}$ part of its former value. Observations, however, show that the actual increase in the amount of the reduction is very small, being on an average only *one-seventh* as great

* Since this article was written I have been informed that the constant 6·36 inches for reducing the Mt. Washington observations to sea-level began to be used March 1st, 1874, and that for the two preceding years the constant 6·31 inches had been used. Thus it appears that for a period of eighteen months, I had made the Mt. Washington barometric observations too low by 0·05 inch, which would indicate an average error of about 0·02 inch for the entire period of the observations. This would correspond to an average error of about 0·001 inch in the column of observed reductions to sea-level, which is so small an error that I have not considered it necessary to re-compute the entire series of observations.

Reduction to sea-level of barometer on Mt. Washington, elevation 6285 fee[t]

Ther.		22·7	22·8	22·9	23·0	23·1	23·2	23·3	23·4	23·5	23·6	23·7	23·8	23·9	24·0	24·1	24
	D	6·886	6·912	[illegible]	6·964	6·990	7·016	7·043	7·070	7·096	7·124	7·150	7·177				
−10°	L	6·875	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	7·087	7·117	7·148	7·176	7·206				
	P	6·779	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	7·016	7·046	7·076	7·106				
	O	[illegible]	7·142	[illegible]	7·150	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]							
	D	6·785	6·811	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·995	7·022	7·050	7·079	7·105			
−5	L	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	7·049	7·079	7·109	[illegible]			
	P	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	7·016	[illegible]			
	O	[illegible]	7·011	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]					
	D	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·987	7·014	7·041		
0	L	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	7·013	7·042	7·072		
	P	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·951	6·980		
	O	6·882	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]					
	D	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·913	6·939		
+5	L	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·918	6·947	6·976		
	P	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·843	6·872	6·901		
	O	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]						
	D	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·771	6·798	6·825	6·851	6·877	
10	L	6·512	6·540	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·827	6·856	6·884	6·912	
	P	6·446	6·474	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·759	6·788	6·816	6·845	
	O	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·741							
	D	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·710	6·737	6·763	6·789	6·
15	L	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
	P	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
	O	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]			
	D	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
20	L	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
	P	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
	O	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		
	D	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·562	6·587	6·612	6·
25	L	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·622	6·649	6·
	P	6·216	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·574	6·602	6·
	O			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·540	
	D	6·186	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·485	6·510	6·
30	L	[illegible]	6·212	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
	P	6·142	6·170	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·522	6·
	O				[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·499	
	D	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·446	6·
35	L	6·108	[illegible]	6·162	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·458	6·485	6·
	P	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·445	6
	O							[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	
	D	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·372	6·
40	L	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
	P	5·997	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
	O							[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
	D	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
45	L	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
	P	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
	O								[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·254	6·
	D	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
50	L	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·252	6·
	P	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·221	6·
	O									[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	
	D	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·142	6·
55	L	[illegible]	5·845	5·871	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·178	6·
	P	5·788	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·148	6·
	O										[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·134	6·
	D	5·751	5·774	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·071	6·
60	P	5·751	[illegible]	5·802	5·827	5·851	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
	L	[illegible]	5·744	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
	O										[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·052	6·
	D	[illegible]	5·706	5·729	5·752	5·774	5·798	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	6·
65	L	5·683	5·710	5·735	5·760	5·785	5·810	5·835	5·860	5·885	5·910	[illegible]	[illegible]	[illegible]	[illegible]	6·035	6·
	P	5·651	5·676	5·701	5·726	5·751	5·776	5·802	5·827	5·852	5·877	5·902	5·927	[illegible]	5·977	[illegible]	6·
	O										5·973	5·973	5·973	[illegible]	5·977	6·001	6·

as that given by the formula of Laplace. The influence of temperature on the reduction to sea-level, as deduced from the observations, differs but little from that given by the formula of Laplace. At the highest temperatures, the observed reduction accords with that computed by the formula when the pressure at sea-level is 29·8 inches; and at the lowest temperatures the agreement occurs when the pressure at sea-level is 30·7 inches. Thus we see that the true reduction of barometric observations to sea-level for Mt. Washington depends mainly upon temperature.

The observed values of the reduction to sea-level given in the table on page 4 are in all cases the means of a considerable number of observations. In some cases the observed values differ very much from the means here given. In order to learn the magnitude of these differences and to study the circumstances under which they occur, I proceeded as follows: I selected all those cases (for the thirty-six months of the tri-daily observations) in which the reduction computed by Dunwoody's Tables differed by 0·25 inch from the observed reduction. The number of these cases was ninety-six. As this table seemed too large for publication, I selected those cases in which the difference amounted to at least 0·3 inch and for these cases the reduction to sea-level was rigorously computed by the formula of Laplace. The results are given in the following table, in which column 1st shows the number of the storm; column 2d shows the date of the observation; column 3d shows the observed height of the barometer on Mt. Washington, not reduced to sea-level. This observed height was obtained by subtracting 6·31 inches from the published heights for all dates preceding March, 1874, and subtracting 6·36 inches for subsequent dates. Column 4th shows the mean temperature of the air column from the summit to the base of the mountain $\left(\frac{2W+B+P}{4}\right)$; column 5th shows the observed reduction to sea-level $\left(\frac{B+P}{2}-W\right)$; column 6th shows the reduction to sea-level computed by Laplace's formula, for a height 6,285 feet, with a barometer and temperature as given in columns 3 and 4; column 7th shows the reduction according to the table on p. 4, in the lines marked O; column 8th shows the difference between the numbers in columns 5 and 6; column 9th shows the difference between the numbers in columns 5 and 7; column 10th shows (in hours) how much the minimum pressure on Mt. Washington occurred later than the half sum of the dates of minimum at Burlington and Portland; column 11th shows the direction and force of the wind on Mt. Washington.

The number of cases in this table is 40, of which 8 occurred in November; 11 in December; 12 in January; 2 in Febru-

Cases in which the reduction to sea-level was unusually great.

No.	Date.			Mt. Was. Barom	Mean Temp.	Red. to sea-lev. Obs	Lapl.	Table	Difference. Lapl.	Table	Low ret'd.	Mt. Wash. Wind.	
1	1872	Nov.	7.3	22·78	30°·2	6·49	6·19	6·41	0·30	0·08	8	N.W.	65
			8.1	·81	31·2	6 54	6·18	6·40	·36	·14	8	N.W.	48
			8.2	·96	34·2	6·49	6·18	6·35	31	·14	8	N.	65
			9.1	23 12	29·7	6·65	6·29	6·42	·36	·23	8	N.W.	58
			9.2	·24	29 7	6·65	6·32	6·43	·33	·22	8	W.	78
2			29.2	22·89	22·0	6·81	6·35	6·53	·46	·28	4	S.W.	35
			30.1	·63	10·2	6·81	6·47	6·72	·34	·09	4	N.E.	45
			30.3	·73	9·6	6·89	6·51	6 73	·38	·16	4	W.	48
		Dec.	1.1	·85	12·6	6·99	6 50	6·68	·49	·31	4	N.	52
3			3.2	23·14	26·5	6·66	6 35	6·48	·31	·18	4	S.E.	60
			4.1	·24	21·7	6·78	6·45	6·55	·33	·23	4	W.	58
4			10.1	22·78	7·2	6·87	6·57	6·76	·30	·11	16	W.	59
5			15.2	23·12	20·0	6·82	6 45	6·57	·37	·25	7	N.	68
6			22.2	·05	— 1·2	7 13	6·77	6 95	·36	18	13	W.	43
7			24.3	·05	—13·0	7·44	7·00	7·22	·44	·22	?	W.	44
8			28.1	22·75	-10·7	7·21	6·89	7·16	·32	·05	15	N.E.	60
9	1873	Jan.	6.2	23·14	15·0	6 96	6·54	6·64	·42	·32	9	W.	60
10			11.3	·06	— 1·2	7·10	6·80	6·95	·30	·15	12	N.	76
11			29.1	·10	— 2·2	7·14	6 83	6·97	·31	·17	4	W.	54
12		Feb.	10.1	22·96	— 2·5	7·18	6·80	6·96	·38	·22	?	N.W.	77
			10.2	23·01	0 7	7·08	6·75	6·89	·33	·19	?	N.W.	103
13		March	16.2	22·78	21·7	6·67	6·32	6·54	·35	·13	13	W.	28
			16.3	·74	17·2	6·88	6 39	6·61	49	27	13	N.W.	56
			17.1	·87	14·2	6·95	6·47	6·65	48	·30	13	W	56
			17.2	23·15	17·7	6·81	6·49	6·60	·32	·21	13	N.W.	25
14			24.1	22·92	8 0	7·07	6·60	6 76	·47	·31	13	W.	48
15		Dec.	10.1	23·33	23·7	6·78	6 44	6·53	·34	·25	9	N.W.	62
16	1874	April	30.2	22·67	23·2	6·72	6·28	6·51	·44	·21	4	N.W.	130
			30.3	·72	23·5	6 70	6 30	6·51	·40	·19	4	N.W.	115
17		Dec.	29.2	23·02	11·2	6·88	6 58	6·70	30	·18	18	N.W.	80
			30.2	·01	- 5·2	7·22	6·87	7·03	35	19	18	N.W.	98
18	1875	Jan.	9.3	22·70	— 8·7	7·26	6 85	7·10	·41	·16	8	N.W.	100
19			14.1	·91	12·0	6·89	6·54	6·69	·35	·20	28	N.W.	70
			14.3	·82	— 4·0	7·13	6·80	6·99	·33	·14	28	N.W.	—
			15.1	·71	—15·5	7·42	6·98	7·29	·44	·13	28	N.W.	—
			15.2	·95	-10·2	7·25	6·95	7·14	30	·11	28	N.W.	—
			15.3	23·00	— 7·6	7·24	6 92	7·09	32	·15	28	N.W.	—
20			17 1	22·91	—11·2	7·28	6·96	7·18	32	·10	12	N.W.	—
21			25.2	[illegible]	13·0	6 91	6·53	6·68	·38	·23	9	N.W.	80
			26.2	·95	0·0	7·14	6·76	6·91	·38	·23	9	N.W.	94

ary; 5 in March and 2 in April. During the six months from May to October inclusive, no case occurred in which the observed reduction differed 0·3 inch from that computed by the Laplace formula, and four-fifths of these cases occurred in the months of November, December and January. Fifteen of these cases occurred at the 7.35 A. M. obs.; 16 occurred at the 4.35 P. M. obs.; and nine at the 11 P. M. obs., indicating that the diurnal change of temperature has an appreciable influence upon this phenomenon, but that it is mainly dependent upon some other cause. It will also be noticed that in every one of

these cases the observed reduction was greater than that computed from the Laplace formula. There is not a single instance in which the observed reduction was 0·3 inch less than that computed from the Laplace formula.

The mean of the numbers in column 8th is 0·362 inch, and the mean of the numbers in column 9th is 0·190 inch, showing that when the reduction is computed from the table on page 4, the average error is but little more than half as great as when computed from the Laplace formula. There are ten cases, out of 3,285 observations, in which the error of the reduction by the Laplace formula exceeds 0·4 inch, and there are only three cases in which the error of the reduction by the table page 4, exceeds 0·3 inch. This table is therefore a great improvement upon Laplace and also upon any other table of reductions hitherto published.

All these cases enumerated on page 6 occurred during the progress of storms which were generally of considerable violence. In every case, the barometric minimum on Mt. Washington occurred later than it did near the level of the sea, the average retardation amounting to more than eleven hours. In most of the cases the barometer at the lower stations had passed the minimum, and in about half of the cases had risen to thirty inches, while the barometer on Mt. Washington had risen comparatively little. In a large part of the cases the temperature was unusually low and the wind on Mt. Washington was very high. In two cases the temperature at Burlington was lower than it was on Mt. Washington, and in other cases the difference of temperature was very small. In 1873, Jan. 29.1, the thermometer at Burlington was 9° lower than on Mt. Washington; on Feb. 10.1, it was 2° lower; in 1872, Dec. 24.3, the temperature at both stations was the same; and in 1873, March 24.1, it was only 2° colder on Mt. Washington than at Burlington. These observations help to explain in a few of the cases, why there was an increased pressure at the lower stations which did not extend to the summit of Mt. Washington. A cold stratum of air whose height was less than 6,000 feet, flowed along the surface of the earth, increasing the barometric pressure at the lower stations, but producing no decided effect upon the pressure at the summit of Mt. Washington.

It will also be observed that in half of these cases the wind on Mt. Washington was from the northwest; and in four-fifths of the cases it was either west or northwest. The velocity of the wind was also remarkable, the average being sixty-six miles per hour, and in four instances the velocity was one hundred miles or more. In 1875, from Jan. 14th to 17th, the velocity of the wind was not reported, but it is presumed to have been about one hundred miles per hour. It is evident, therefore,

that these cases of low pressure on Mt. Washington were generally the result of great storms in progress, and in most of the cases the violence of the storm had ceased at the lower stations while it continued unabated on Mt. Washington. The Danish weather maps which show the isobars for the Atlantic Ocean since March, 1874, assist us in understanding the cause of these high winds on Mt. Washington. They show that an area of low pressure prevailed on the east side of the mountain, generally near Nova Scotia or Newfoundland; and the winds on Mt. Washington were controlled by this low area long after the high winds at Burlington and Portland had subsided.

We thus find that if the barometric observations on Mt. Washington are reduced to sea-level by the table on page 4, the results will rarely differ one-tenth of an inch from actual observations made near sea-level. Exceptional cases will sometimes occur; but great anomalies are confined to the colder months of the year, and seldom occur except during the progress of violent storms.

In order to ascertain whether the law respecting the reduction of barometric observations to sea-level, which has been discovered for Mt. Washington, holds true for other mountains, I made a comparison of the observations on Pike's Peak, when reduced to the altitude of Denver. The altitude of Pike's Peak, as determined by a preliminary computation which differs slightly from the final result given on page 18 is 14,056 feet, and that of Denver is 5,262 feet. The materials employed for this comparison consisted of the tri-daily observations from November, 1873, to January, 1875, and from January, 1877, to July, 1877 (22 months), and the monthly means from November, 1873, to June, 1879, published in the annual reports of the Signal Service. These materials were reduced in the manner already described in the case of Mt. Washington. The table on page 9 shows the reduction from Pike's Peak to Denver for a range of the barometer from 17·1 inches to 18·2 inches on Pike's Peak, and for a mean temperature of the air-column between the two stations from $-10°$ to $+60°$. Corresponding to each temperature are two horizontal lines, one of which is marked L, showing the reduction computed from Guyot's Tables based on the formula of Laplace (Guyot's Met. Tables, series D, p. 33), and the other, marked O, shows the reduction deduced from actual observations.

We see from this table that the increase in the value of the reduction to sea-level resulting from an increase of pressure on Pike's Peak is very small, being, on an average, less than *one-fifth* of that computed from the formula of Laplace. In this particular the results accord very closely with those before found for Mt. Washington. The influence of temperature

upon the amount of the reduction differs somewhat from that given by the formula. While the pressure at Pike's Peak remains unchanged, the observed change in the reduction to Denver, resulting from a change of temperature, is 33 per cent less than the computed reduction. At the highest temperatures the observed reduction accords with that computed by the formula when the barometer at Denver stands at 24·95 inches; and at the lowest temperatures the agreement occurs when the barometer at Denver stands at 24·45 inches.

Reduction of barometer from Pike's Peak (elevation 14,056 *feet) to Denver (elevation* 5,262 *feet).*

Therm.		17·1.	17·2.	17·3.	17·4.	17·5.	17·6.	17·7.	17·8.	17·9.	18·0.	18·1.	18·2.
−10°	L	7·651	7·696	7·741	7·786	7·830	7·875	7·920	7·965	8·010	8·055	8·100	8·145
	O	7·565	7·572										
− 5	L	7·540	7·584	7·628	7·673	7·717	7·762	7·806	7·850	7·894	7·938	7·982	8·027
	O	7·511	7·517	7·523									
0	L	7·433	7·477	7·520	7·564	7·607	7·651	7·694	7·738	7·781	7·825	7·868	7·912
	O	7·436	7·440	7·442	7·442	7·445							
+ 5	L	7·329	7·372	7·415	7·458	7·501	7·544	7.586	7·629	7·672	7·715	7·758	7·801
	O	7·353	7·359	7·363	7·364	7·364	7·364						
10	L	7·227	7·269	7·312	7·354	7·396	7·439	7·481	7·523	7·566	7·608	7·651	7·693
	O	7·278	7·284	7·291	7·291	7·291	7·290	7·289	7·287				
15	L	7·128	7·170	7·212	7·253	7·295	7·337	7·378	7·420	7·462	7·504	7·546	7·588
	O	7·212	7·216	7·218	7·223	7·228	7·228	7·228	7·228				
20	L	7·032	7·073	7·114	7·155	7·197	7·238	7·279	7·320	7·361	7·402	7·443	7·485
	O		7·12[illegible]	7·155	7·170	7·177	7·180	7·186	7·188				
25	L	6·938	6·979	7·019	7·060	7·101	7·[illegible]41	7·182	7·222	7·263	7·303	7·344	7·385
	O			7·087	7·102	7·120	7·126	7·127	7·130	7·133			
30	L	6·847	6·887	6·927	6·967	7·007	7·047	7·088	7·128	7·168	7·208	7·248	7·288
	O				7·033	7·043	7·064	7·067	7·070	7·076			
35	L	6·758	6·798	6·837	6·877	6·916	6·956	6·995	7·035	7·074	7·114	7·153	7·193
	O				6·968	6·976	6·993	7·010	7·017	7·030	7·036		
40	L	6·671	6·710	6·749	6·788	6·827	6·866	6·905	6·944	6·983	7·023	7·062	7·101
	O					6·914	6·930	6·939	6·957	6·972	6·980		
45	L	6·587	6·626	6·664	6·703	6·741	6·780	6·818	6·857	6·895	6·934	6·972	7·011
	O						6·860	6·867	6·884	6·908	6·920		
50	L	6·505	6·543	6·581	6·619	6·657	6·695	6·733	6·771	6·809	6·848	6·886	6·924
	O							6·797	6·809	6·842	6·857	6·860	
55	L	6·425	6·463	6·500	6·538	6·575	6·613	6·650	6·688	6·725	6·763	6·801	6·838
	O								6·754	6·772	6·795	6·802	
60	L	6·346	6·383	6·420	6·457	6·494	6·532	6·569	6·606	6·643	6·680	6·718	6·755
	O									6·733	6·738	6·744	6·750

In order to test the preceding results under different circumstances I selected two stations near the Pacific coast, viz: Sacramento and Summit. Sacramento is situated in lat. 38° 35′, long. 121° 31′, and is elevated 31 feet above the sea. Summit is situated on the Central Pacific Railroad in lat. 39° 20′, long.

120° 5′, and is elevated 7,017 feet above the sea. At these stations meteorological observations were made three times a day for three years in connection with the Geological Survey of California under the direction of Prof. Josiah D. Whitney. The monthly means of the barometer and thermometer were published by Prof. Whitney in a volume entitled "Contributions to Barometric Hypsometry," and the original observations have been placed in my hands by Prof. Whitney. For the purpose of comparing these observations, a table was prepared showing for each day of the three years—1. the height of the barometer at Summit according to the mean of the three daily observations reduced to 32° F.; 2. the mean of the temperatures at Summit and Sacramento for each day, according to the three daily observations; and 3. the difference between the mean barometric heights at Summit and Sacramento for each day. These results were then divided into classes according to temperature in such a manner that each class should include a range of five degrees, and the middle temperature should be some multiple of five. The observations of each of these classes were then compared in respect to barometric pressure at Summit, so that all those observations which were made at nearly the same pressure were grouped together, and an average was taken of the numbers in each of these groups. In this way I obtained the reduction to Sacramento corresponding to a considerable range of temperature and pressure. The inequalities of the resulting numbers were somewhat smoothed down by applying the method described on page 3. The final results are given in the table on page 11 which shows the reduction of the barometer from Summit to Sacramento for pressures ranging from 22·7 to 23·6 inches; and for temperatures of the air-column from Summit to Sacramento ranging from 25° to 80° F. Corresponding to each temperature are given two horizontal lines marked L and O; the former shows the reduction computed from the formula of Laplace, the latter shows the reduction deduced from the actual observations.

An examination of this table shows that the reduction of the barometer from Summit to Sacramento instead of *increasing* with an increase of pressure, as required by the formula of Laplace, invariably *decreases;* and the average observed decrease is seven-eighths of the increase computed from the formula; and for all temperatures above 40° the observations show a decrease in the amount of the reduction *fully equal* to the increase computed from the formula. This result shows that while the formula of Laplace gives the reduction to sea-level with tolerable accuracy when the atmosphere is nearly in a condition of equilibrium, it gives very erroneous results when the atmosphere is greatly disturbed. While the pressure at

Summit remains unchanged, the observed change in the reduction to Sacramento resulting from a change of temperature is 41 per cent less than that computed from the formula; but at all temperatures the observed reduction accords nearly with that computed from the formula when the barometer at Sacramento stands at 29 9 inches.

Reduction of barometer from Summit (elevation 7,017 feet) to Sacramento (elevation 31 feet).

Temp.		22·7.	22·8.	22·9.	23·0.	23·1.	23·2.	23·3.	23·4.	23·5.	23·6.
25°	L	7·057	7·088	7·119	7·150	7·181	7·212	7·243	7·274	7·305	7 336
	O				7·037	7·016	7·000	6·983			
30	L	6·967	6·997	7·028	7·059	7·089	7 120	7·151	7·182	7 212	7·243
	O			6·997	6·989	6 975	6·953	6·936	6·928		
35	L	6·879	6·910	6 940	6·970	7 001	7·031	7·061	7 091	7·122	7·152
	O	6 987	6·976	6 967	6 948	6·930	6·911	6 889	6·867	6 841	
40	L	6·794	6 824	6 854	6 884	6·914	6 944	6 974	7·003	7 033	7·063
	O		6·923	6·919	6 906	6 889	6 870	6 844	6 811	6 787	6 750
45	L	6·711	6 740	6 770	6 799	6 829	6 858	6 888	6 917	6 947	6·977
	O			6·895	6·884	6 851	6 825	6 798	6 766	6 730	6·695
50	L	6·629	6·658	6·688	6·717	6 746	6·775	6 804	6 834	6 863	6 892
	O				6·856	6 810	6 765	6·737	6·703	6·672	6·650
55	L	6·549	6·578	6·607	6·636	6 665	6 694	6 723	6 752	6·781	6·810
	O					6·720	6·698	6 667	6 633	6 597	
60	L	6·472	6·501	6·529	6·558	6·586	6·615	6·643	6·672	6 700	6·729
	O					6 652	6 626	6 606	6 575	6 538	
65	L	6·397	6·425	6 454	6·482	6 510	6 538	6 566	6 594	6 623	6 651
	O						6 586	6 560	6 530	6 506	
70	L	6·323	6·351	6·379	6·406	6 434	6 462	6 490	6·518	6 546	6·574
	O						6·560	6·511	6·487	6·449	
75	L	6·251	6 278	6·306	6·333	6·361	6 389	6 417	6·445	6·472	6·499
	O							6·481	6·438	6 401	
80	L	6·180	6 207	6·234	6·262	6·289	6·316	6·343	6·371	6·398	6 425
	O									6 366	

In order to study this question under a still greater variety of circumstances I selected two elevated stations in Europe, viz: Grand St. Bernard and Colle di Valdobbia. The former station is situated in a pass over the Alps, at an elevation of 2.462 meters above the sea, and the station selected for comparison is Geneva, distant 55 English miles from St. Bernard and elevated 407 metres above the sea. The observations are published in the Bibliothèque Universelle de Genève, and I have employed the observations of three years, viz: 1877, 8 and 9. The mode of reducing the observations was similar to that described on page 10. The results are shown in the table on page 12 which exhibits the reduction computed by the Laplace formula from Delcros' Tables (Smithsonian Tables, series D, page 11), for a range of the barometer from 546 to 573 millimeters, and for a

range of temperature from $-12°$ to $+18°$ centigrade. The same table shows the observed reduction as far as the range of the observations will permit.

Reduction of barometer from Grand St. Bernard (elevation 2,462 *meters) to Geneva (elevation* 407 *meters).*

Ther. Cent.		546.	549.	552.	555.	558.	561.	564.	567.	570.	573.
−12°	L	169·44	170·37	171·31	172·24	173·18	174·11	175·05	175·99	176·92	177·84
	O			169·83	170·79						
10	L	167·84	168·77	169·70	170·62	171·55	172·47	173·40	174·33	175·26	176·17
	O	167·02	167·99	168·89	169·64	170·43	171·19				
8	L	166·26	167·18	168·10	169·02	169·94	170·85	171·77	172·69	173·61	174·52
	O	165·94	166·88	167·76	168·60	169·42	170·38	171·36			
6	L	164·71	165·62	166·52	167·43	168·34	169·25	170·16	171·17	171·98	172·88
	O	164·66	165·52	166·37	167·20	167·90	168·69	169·57	170·51		
4	L	163·20	164·10	164·99	165·89	166·79	167·69	168·58	169·48	170·38	171·28
	O	163·04	163·88	164·72	165·55	166·33	167·08	167·84	168·46		
− 2	L	161·70	162·59	163·48	164·37	165·26	166·15	167·04	167·93	168·82	169·71
	O		162·00	162·95	163·90	164·71	165·49	166·27	166·97	167·63	
0	L	160·22	161·10	161·98	162·87	163·75	164·64	165·52	166·41	167·29	168·17
	O		160·40	161·46	162·33	163·18	163·99	164·80	165·61	166·65	
+ 2	L	158·78	159·65	160·53	161·41	162·28	163·16	164·03	164·91	165·78	166·65
	O			159·79	160·85	161·72	162·55	163·37	164·19	164·90	165·57
4	L	157·36	158·23	159·10	159·97	160·83	161·70	162·57	163·44	164·30	165·17
	O				159·43	160·40	161·37	162·12	162·89	163·67	164·57
6	L	155·98	156·84	157·70	158·55	159·41	160·27	161·13	161·99	162·84	163·70
	O					159·77	160·52	161·06	161·80	162·58	163·33
8	L	154·62	155·47	156·32	157·17	158·02	158·87	159·72	160·57	161·42	162·27
	O						159·30	160·03	160·75	161·51	162·17
10	L	153·28	154·11	154·96	155·80	156·64	157·49	158·33	159·18	160·02	160·87
	O						158·00	158·79	159·63	160·38	161·16
12	L	151·96	152·79	153·63	154·46	155·30	156·14	156·97	157·81	158·64	159·48
	O							157·77	158·46	159·14	159·87
14	L	150·67	151·50	152·33	153·15	153·98	154·81	155·64	156·47	157·29	158·12
	O								157·08	157·77	158·50
16	L	149·39	150·21	151·03	151·85	152·67	153·49	154·32	155·14	155·96	156·78
	O								155·31	156·29	157·26
18	L	148·12	148·93	149·75	150·56	151·38	152·19	153·01	153·82	154·63	155·45
	O									154·83	155·70

We see that the observed reduction accords with the computed reduction much better than in either of the preceding cases. The change in the value of the reduction due to a change either of the barometer or thermometer is, however, a little less than that computed from the formula.

The other elevated station selected is the Colle di Valdobbia, situated about 10 English miles south of Monte Rosa, at an elevation of 2,485 meters above the sea, and the station selected for comparison is Alessandria, distant about 70 English miles, and elevated 98 meters above the sea. The observations are

published in Bullettino Meteorologico dell' Osservatorio in Moncalieri, and the years selected for comparison are those of 1877, 8 and 9. The observations were reduced in the manner already described on page 10, and the results are shown in the following table which is arranged in the same manner as the preceding table.

duction of barometer from Colle di Valdobbia (elevation 2,485 meters) to Alessandria (elevation 98 meters).

		543.	546.	549.	552.	555.	558.	561.	564.	567.	570.	573.
10	L	198·34	199·44	200·53	201·63	202·73	203·82	204·92	206 02	207·12	208·21	209·31
	O				197·00							
8	L	196 44	197·53	198·61	199·70	200 79	201·87	202·96	204·05	205·14	206·22	207·31
	O		197·49	197 50	197·50	197·49	197·47					
6	L	194·57	195·65	196·72	197·80	198·88	199 95	201·03	202·11	203·19	204·26	205·34
	O		194·67	195·48	196·07	196 65	197·13					
4	L	192·74	193·81	194 87	195·94	197·00	198·07	199 14	200·20	201·27	202·33	203·40
	O		192 75	193·83	194·68	195·47	196·10	196 41	196·52			
2	L	190·94	192·00	193·05	194·11	195·16	196·22	197·28	198·33	199 39	200·44	201·50
	O	190·46	191·47	192·49	193 28	193·93	194·46	194·84	195·13	195·28		
0	L	189·17	190·22	191·26	192·31	193 35	194·40	195 45	196·49	197·54	198·58	199·63
	O		190 85	191·54	192·04	192·53	193 04	193·55	194·01	194·27	194·53	
2	L	187·43	188·47	189·50	190·54	191·58	192 61	193·65	194 69	195·73	196·76	197·80
	O			190 45	190·93	191·46	192·01	192·55	193·06	193·57	194·12	
4	L	185·73	186·76	187·79	188·81	189·84	190 87	191·90	192·93	193·95	194·98	196·01
	O			189 20	189·82	190 37	190 98	191·56	192·06	192·58	193·13	
6	L	184·07	185·09	186·11	187·12	188·14	189·16	190·18	191·20	192·21	193 23	194·25
	O				188·41	189 08	189·68	190·34	190 88	191·37	191·87	
8	L	182·43	183·44	184·45	185·45	186 46	187·47	188 48	189 49	190·49	191·50	192·51
	O					187 69	188·37	189·01	189·66	190 20	190·70	
10	L	180·81	181·81	182·81	183·81	184·81	185·81	186·81	187·81	188·81	189 81	190·81
	O						186·98	187 67	188·37	188 94	189·59	
12	L	179·21	180·20	181·20	182 19	183·18	184·17	185·17	186·16	187·15	188·15	189·14
	O						185·63	186·39	187·04	187 70	188·20	
14	L	177·64	178 62	179·61	180·59	181·58	182·56	183·55	184·53	185·52	186·50	187·49
	O							185·14	185·78	186 41	187·06	
16	L	176·11	177·08	178·05	179·03	180·01	180 98	181 96	182·94	183·92	184·89	185·87
	O							183·87	184·52	185·33	185·82	186·22
18	L	174·60	175·57	176·54	177·50	178 47	179·44	180 41	181·38	182·34	183 31	184 28
	O								184·05	184·21	184·42	184·42
20	L	173·11	174·07	175·03	175·99	176 95	177·91	178·87	179·83	180·79	181·75	182·71
	O									183 19	183·14	183·15

This table shows results quite different from those of the preceding table. While the pressure at the upper station remains unchanged, the observed change in the reduction to the lower station resulting from a change in the temperature of the air-column is 30 per cent less than that computed from the Laplace formula with the constants of Delcros. While the

mean temperature of the air-column remains unchanged, the observed change in the reduction to the lower station resulting from a change of pressure at the upper station, is only one-half of that computed from the formula. Thus we see that the reduction of barometric observations to sea-level follows different laws at different localities. The following table shows a summary of these results for these five mountain stations:

Stations.	Change of reduction depending upon Thermometer.	Barometer.
Mt. Washington,	0·973	+0·142
Pike's Peak,	·675	+ ·195
Summit, Cal.,	·590	— ·866
Grand St. Bernard,	·912	+ ·989
Colle di Valdobbia,	·695	+ ·500
Mean,	·769	+ ·192

Column 1st shows the names of the mountain stations; column 2d shows the average values of the observed change in the reduction to the lower station resulting from a change in the temperature of the air-column, and compared with the change computed from the formula; column 3d shows the average value of the observed change in the reduction resulting from a change of pressure at the upper station, and compared with the computed change.

A comparison of these results shows that the temperature coëfficient employed by Delcros and Guyot is too large; and the observed values of the reduction to sea-level would in most cases be somewhat better represented by assuming a larger value for the coëfficient 18336 meters or 60158·6 English feet adopted by Laplace from the observations of Ramond made more than 75 years ago. It does not seem possible, however, by any change of these coëfficients to modify the Laplace formula so that it may satisfactorily represent the results at all of the preceding stations; or even at a single station for all variations of temperature and pressure.

The Laplace formula assumes that the atmosphere has attained a condition of equilibrium, and in such a case it gives the reduction to sea-level with tolerable accuracy. The average of a long series of observations represents approximately such a condition of equilibrium; but in the daily observations this equilibrium is very much disturbed. The mean between the temperatures at the upper and lower stations does not represent the average temperature of the intermediate column of air; and when the atmosphere is in rapid motion the downward pressure is modified by the earth's rotation in a manner not represented by the Laplace formula. There is no doubt that the formulæ of reduction now employed may be considerably improved; but it does not seem possible that any single

formula with constant coëfficients should provide for the immense variety of conditions which prevail in the neighborhood of mountain stations; and we may be compelled for each mountain region to adopt tables founded upon a direct comparison of observations made at stations of different elevations and not very remote from each other.

I have endeavored to represent by formulæ of a different kind the observed values of the reduction given in the preceding tables. They may all be rudely represented by expressions of the form

$$\text{Reduction} = X - Y\,d\,T + Z\,d\,B,$$

where X represents the value of the reduction for a mean temperature and pressure; Y represents the change in the reduction caused by an increase of 1° in temperature; and Z represents the change caused by an increase of 0·1 inch in the barometer; but this formula is not sufficiently accurate to be of any use. The formula is improved by adding a term representing the variability of the temperature correction. The following expression represents very well the observed values of the reduction for Mt. Washington.

$$\text{Reduction} = 6{\cdot}499 - 0{\cdot}0164\,d\,T + 0{\cdot}0039\,d\,B + 0{\cdot}07\,\sin\,(4^\circ{\cdot}235\,dT - 41^\circ{\cdot}175),$$

where dT represents the excess of the temperature of the air column above 28° F., and $d\,B$ represents the excess of the barometer on Mt. Washington above 23·5 inches. By adding another term representing the variability of the barometric correction the formula may be made to represent the observations still more closely; but this term is so small in amount that it cannot be satisfactorily determined without observations continued for a longer period.

The observed values of the reduction given for Mt. Washington may be condensed into a small table which shall represent these values with differences perhaps no greater than their probable errors. For this purpose I take the mean of all the observed values corresponding to the temperature –10°, and also determine the average correction at that temperature for a change of 0·1 inch in the barometer. I do the same for the temperature –5°, and so on through the table. By applying the proper barometric correction, these averages are all reduced to the barometric height 23·5 inches.

In the following table, column 1st shows the degrees of the thermometer (Fah.) from –10° to + 80°; column 2d shows for each temperature the mean reduction to sea-level when the barometer on Mt. Washington stands at 23·5 inches; and column 3d shows the correction due to a change of 0·1 inch in the

barometer; column 4th was obtained in a similar manner and shows the reduction from Pike's Peak to Denver when the barometer on Pike's Peak stands at 17·6 inches, and column 5th shows the correction due to a change of 0·1 inch in the barometer; column 6th shows the reduction from Summit to Sacramento when the barometer at Summit stands at 23·3 inches, and column 7th shows the correction for 0·1 inch in the barometer, which correction is negative when the pressure increases.

Reduction of barometric observations.

Therm. Fahr't.	Mt. Washington and sea-level. Barometer 23·5 inches.		Pike's Peak and Denver. Barometer 17·6 inches.		Summit and Sacramento. Barometer 23·8 inches.	
	Reduction.	Correction 0·1 inch bar.	Reduction.	Correction 0·1 inch bar.	Reduction.	Correction 0·1 inch bar.
−10°	7·158	·0021	7·600	·0070		
− 5	7·054	·0056	7·541	·0060		
0	6·943	·0061	7·448	·0022		
+ 5	6·831	·0039	7·367	·0022		
10	6·732	·0015	7·290	·0013		
15	6·657	·0032	7·227	·0023		
20	6·586	·0032	7·179	·0108		
25	6·520	·0047	7·112	·0077	6·982	−·0180
30	6·450	·0068	7·055	·0086	6·942	−·0138
35	6·374	·0074	6·993	·0113	6·888	−·0182
40	6·307	·0035	6·929	·0132	6·833	−·0216
45	6·239	·0024	6·858	·0150	6·791	−·0286
50	6·173	·0024	6·786	·0157	6·742	−·0343
55	6·107	·0027	6·725	·0160	6·663	−·0307
60	6·037	·0026	6·716	·0057	6·599	−·0285
65	5·968	·0022			6·558	−·0267
70					6·520	−·0370
75					6·480	−·0400
80					6·432	

The irregularities of these numbers may be diminished by taking the mean of each three consecutive numbers in each of the vertical columns; but I prefer to leave the numbers precisely as they have been derived from the preceding tables.

If the formulæ of reduction to sea-level hitherto employed are admitted to be unsatisfactory for great elevations, it does not seem safe to conclude that they are correct for small elevations. For elevations less than 1000 feet the error of reduction is less palpable than for an elevation of 6000 feet, but it is probable that the error is only proportionally diminished.

Height of the Signal Service stations.

In my 12th paper I gave the results of some computations which indicated considerable errors in the assumed heights of some of the stations of the Signal Service. The publication

in the Annual Report for 1879 of the mean barometric heights for all the stations of the Signal Service without reduction to sea-level, affords materials for a new determination of these heights. The following table shows all the stations of the Signal Service whose elevation above the sea is more than 1000 feet, and for which the mean heights of the barometer are given for a series of years. Column 1st shows the name of the station; columns 2d and 3d the latitude and longitude; column 4th the elevation in feet as assumed by the Signal Service; column 5th the mean height of the barometer for the entire year, as given in the Report for 1879, page 451; column 6th shows the mean temperature of the station; column 7th shows the mean temperature at sea-level under the station, determined in the manner described in my 12th paper; column 8th shows the mean height of the barometer for each station at sea-level. These numbers were determined in the following manner. For all stations whose elevation was less than 1000 feet I took the mean height of the barometer according to the reduction adopted by the Signal Service; and for stations elevated more than 1000 feet I made the reduction according to the elevations as I had previously determined them. I took the mean barometric heights for all the meteorological stations of the Dominion of Canada, so far as they are published in the official Reports. For various additional stations in the vicinity of the United States, I took the barometric heights from Buchan's Memoir on the Mean Pressure of the Atmosphere, in the Transactions of the Royal Society of Edinburgh, vol. xxv. These numbers were all represented as accurately as possible by isobars drawn upon a chart of the United States. This chart is exhibited upon a greatly reduced scale on Plate I. From this chart the most probable mean pressure for each station was derived, and the results are given in column 8th of the table. Column 9th shows the altitude of each station computed from the data here given according to Guyot's Tables; and column 10th shows the altitude computed from Williamson's Tables which are founded upon Plantamour's formula. The height of Pike's Peak was obtained by computing first its elevation above Denver from the observed values of the pressure and temperature at those stations, and adding this re[illegible] the height of Denver computed from the data conta[illegible] table for [illegible]on.

[illegible] several of these stations ha[illegible] [illegible]asurement so far as [illegible] 1200 [illegible] [illegible]hat reached by the part[illegible] [illegible] They fo[illegible]

[illegible]

the differences between the numbers in columns 4 and 9 for those stations whose heights have been determined by direct measurement, we shall find that the sum of the positive differences is about equal to the sum of the negative differences, which seems to indicate that Guyot's Tables give better results than Williamson's Tables, and that they may be depended upon for heights deduced from the mean of a long series of barometric observations.

Stations of the U. S. Signal Service whose elevation above the sea is more than 1,000 *feet.*

Station.	Lat.	Long.	Elevat'n. Sig. Ser.	Mean Barom.	Temperature.		Barom. Sea-lev.	Elevation.	
					Station.	Sea-lev.		Laplace.	Plant'r.
	°	°			°	°			
Pike's Peak	38·8	105·0	14150	17·750	19·2	55·4	30·032	14054	14116
Santa Fé	35·7	106·2	6851	23·265	48·8	58·8	30·023	7011	7029
Mt. Washing'n	44·3	71·3	6285	23·626	25·9	45·5	29·973	6286	6319
Cheyenne	41·2	104·7	6057	24·015	44·8	51·4	30·031	6068	6093
Pioche	38·0	114·4	5778	24·039	54·9	55·9	30·037	6143	6165
Virginia City	45·3	112·0	5480	24·238	41·0	49·6	30·039	5788	5810
Denver	39·7	105·1	5269	24·779	49·3	54·2	30·032	5260	5278
Salt Lake City	41·2	112·0	4362	25·642	52·4	53·3	30·042	4342	4355
Winnemucca	41·0	117·7	4335	25·621	50·2	54·1	30·050	4366	4379
Boise City	43·7	116·1	2877	27·144	52·4	52·4	30·060	2795	2804
North Platte	41·1	100·9	2838	27·057	48·7	51·7	30·029	2841	2851
Dodge City	37·6	100·1	2486	27·381	54·1	56·6	30·033	2549	2557
Bismark	46·8	100·6	1704	28·154	41·0	42·7	30·010	1708	1716
Yankton	42·7	97·5	1275	28·718	45·8	49·5	30·023	1205	1208
Fort Sill	34·7	98·5	1100	28·779	60·6	60·6	30·032	1188	1192
Omaha	41·3	96·0	1077	28·876	49·8	50·8	30·030	1068	1072

In preparing the materials for this article, I have been assisted by Mr. Henry A. Hazen, a graduate of Dartmouth College of the class of 1871; and Mr. Orray T. Sherman, a graduate of Yale College of the class of 1877.

ART. II.—*Coal Dust as an element of danger in Mining;* by Rev. H. C. HOVEY, A.M.

CHEMICAL action is often induced in heaps of slack, such as exist in thick coal workings, and the heat evolved may be For eib to cause ignition. The danger is greatly increased palpable broken coal is comminuted and floats in the air in that the errol is which under various conditions may undergo

Height oj nts show th when the particles are so
In my 12th paper I gave the results of a safety-lamp, an which indicated considerable errors in the asBauerman states some of the stations of the Signal Service. Thesions have s of a blast,

even in cases where no fire-damp was present in the workings." The influence of coal dust in spreading the effects of gas explosions is one of the subjects of investigation by the royal commission on accidents in mines, now sitting in England.

My object in this article is to lay before the public, by permission of Mr. Edwin Gilpin, Inspector of Mines for Nova Scotia, the results of his investigation into the part played by coal dust in spreading and augmenting the late explosion in the Albion mines.

The seam is well-known as one of the largest in the world, being thirty-seven feet in thickness, and spreading over a large extent of ground. Many million tons of coal have been extracted from the various pits, since work was begun in 1807, and the mining establishment has long been regarded as one of the most complete that could be devised. The pit in which the explosion occurred on the 12th of November, 1880, was nearly 1000 feet deep, and was ventilated as thoroughly as possible by a large Guibal fan, capable of circulating 120,000 cubic feet of air per minute through the ramifications of the mine. Shortly before the accident referred to, I went entirely through the colliery, in company with Mr. Gilpin and the overman, and we remarked the perfection of the ventilation, and the consequent absence of deleterious gases, even in the remotest bords. On the morning of the disaster, the night watchman reported the mine to be free from gas, except in small and harmless quantities. From what source, then, originated the series of explosions, that began within an hour from the time when this report of entire safety was made, and continued at intervals until the mine became a furnace, whose flames could be subdued only by emptying into its burning chambers the waters of an adjacent river? Was there some sudden exudation of gas from the solid coal, or was this explosion due to the firing of coal dust from a safety-lamp or the flame of a blast?

None of the forty-four men who witnessed the beginning of the catastrophe escaped to explain the mystery; those rescued from more distant galleries had but conjectures to offer; and the only facts definitely ascertained were gathered by an exploring party led by Mr. Gilpin, who, shortly after the original explosion, and at the risk of life, descended into the pit and penetrated as far into the workings as the after-damp wo[illegible] allow. The locality where the unfortunate workme[illegible] they tried to save were known to be was 1200 [illegible] the shaft; and the point reached by the part[illegible] 600 yards in that dire[illegible] They fo[illegible] of men and horse[illegible] others by the [illegible] was the spli[illegible]

and the conclusion was plainly justifiable that the flame of the explosion had not extended thus far.

The walls of the galleries had been swept clear of timber, and presented the appearance of having been brushed with a broom. Volumes of coal dust had been driven along by the force of the blast, and lay in waves and drifts on the floor of the levels, into which the party sank to their knees. It was found that clouds of the finer particles had been carried to the shaft and beyond it into the main north level, where a secondary explosion had taken place, demolishing the "lamp cabin," burning the horses between the shaft and the cabin, and fatally burning the man whose business it was to clean and distribute safety lamps to the miners.

Secondary explosions caused by extracted or generated gas are nearly always in the vicinity of the first one; but here is a case where the second was half a mile from the first, with an intervening space of at least a quarter of a mile known to have been free from flame, and presumed to be free from gas, because men were in it with lamps which showed no indications of its presence.

Water was continually trickling down the shaft, and the levels for some distance around were very wet, hence the dust, as soon as it touched the wet walls would be made innocuous; but the fine, dry particles of carbon that were driven on into the lamp cabin were ready for ignition. It had been the custom for years to keep an oil lamp burning openly here, as the proximity of the shaft and consequent purity of the air made the practice, under ordinary circumstances, perfectly safe. But on this occasion it seems to be certain that the ignition of the coal dust caused a second explosion; and it is probable that the same agency was efficient in producing, or at least augmenting, the subsequent explosions that made it necessary to flood the whole mine. It was as if the wadding of a gun were composed of an inflammable material, which on firing the charge doubled its effect. It should also be noted that, as a rule, the Albion mines were very dry, except in portions nearest the shaft, and the accumulation of dust was very great.

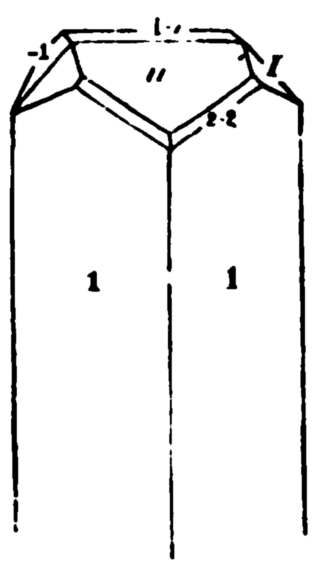
I have only aimed to publish the facts, hoping that some one else may explain on chemical principles the remarkable exhibition of force, as well as of heat, accompanying the ignition of an impalpable and homogeneous powder. Professor Abel's [illegible]ents have shown that even finely powdered slate will [illegible]me of gas explosions; and it is alleged that there [illegible]nent explosions of flour dust in large mills in [illegible] States. In view of these facts the matter, [illegible]worthy of attention [illegible] for that very

In my 12th pape[r] ... which indicated considera[ble] ... in the ... some of the stations of the Signal Service.

II.—*Notes on Mineral Localities in North Carolina*; by WM. EARL HIDDEN.

AZITE *from Milholland's Mill, Alexander Co.*—In August 80) I obtained at this locality some very beautiful crys- geniculated rutile, which had been found there loose in . Permission having been obtained to work the prop- succeeded on the first day's working* in finding these *in situ.* In connection with the work I "panned down" f the loose vein material, and in this manner the mona- stals were first discovered. There is every probability the work at the locality is continued the monazite will ıd in place in the vein. The rock is a garnetiferous chist. The vein (or pocket as it may yet prove is about a foot wide at its widest and thus far has ncovered only about eight feet. My operations were rily limited, and the locality merits further examina- The associated minerals are *muscovite* (?), emerald green ı the prism, very abundant and making up 95 per cent vein, crystals thin hexagonal tables and unusually ; *quartz crystals*, elongated prisms commonly doubly ıted and in parallel groupings, often cavernous; *rutile*, eniculated and splendent; some decomposed pyrites with containing native sulphur; a few pseudomorphs of after siderite, in rhombohedrons having the basal and hedral planes.

ncentrating by "panning," say 15 lbs. of the loose vein l, many hundred minute crystals of monazite would be perhaps only a half a dozen of which exceed $\frac{1}{20}$th inch in diameter; rarely, were found of $\frac{1}{4}$th inch in length. Under croscope, the majority of the minute te crystals are seen to be perfectly trans- and of a topaz color. The planes are ghly polished and lustrous. The crystals formly long prismatic with modified ter- ns, the prism having the shape of an homboid, thus differing from those pre- figured. The adjoining figure represents



nmon form with what are supposed to be the prop- s of the planes. One of the monazites partly enclose al of mica, which fact would point conclusively nation in the vein and also to its later formati

oils of this region are the result of decomposition and disintegr it is consequently an easy task to discover the source of the surface.

The monazite of this locality, as regards occurrence and form, is essentially the *turnerite* of Levy, which has been shown to be identical with monazite, as was long ago suggested by Prof. J. D. Dana. The mode of occurrence and the associated minerals are nearly identical with the Tavetsch, Switzerland, locality; the titanic acid here taking the form of rutile instead of octahedrite. An analysis by Dr. J. Lawrence Smith is now under way, and the crystallography and general physical characters of the mineral will be described by Dr. E. S. Dana.

*Other localities for monazite.**—In Burke County, monazite is very abundant, particularly at J. C. Mill's gold mine in the Brindletown district. I obtained over fifty pounds of gravel washings from this mine that afforded sixty per cent of monazite. Fourteen ounces of chemically pure monazite were obtained here by sifting old "tailings" and picking out the largest crystals; these were sent to Mr. T. A. Edison, who desired the mineral for the thorina which it was supposed to contain.

The crystals are usually well formed and vary considerably in habit. Figs. 446 and 448, Dana, are common; they are usually very small, though some were found here of $\frac{1}{4}$th inch in diameter. The color is light brown. The common occurrence of this mineral in the gold gravels of North Carolina is worthy of note. I believe that pannings from any of the streams where the local rocks are mica schists would bring it to light. In the auriferous gravels of McDowel, Rutherford, Burke and Polk Counties, N. C., it was noticed in every "panning."

In Mitchell County, at the Deake mica mine, I found well formed crystals of monazite *in situ* in mica schist. They were of uncommon size. One measured $1\frac{1}{2}$ inches long by $\frac{1}{2}$ inch in width, and was one of a group. Half a pound of crystals were obtained in all. They were partly coated with autunite, and were intimately associated with uraninite, gummite, garnet, etc. The characteristic perfect basal cleavage was commonly observed at this locality. In Yancey County, at the Ray mica mine on Hurricane Mountain, I found monazite in white orthoclase. The crystals were very fine, and complex in form; specific gravity 5·243. Dr. F. A. Genth has been at work for some years on the monazite of North Carolina and has separated over a pound of the oxalates of the rare earths of the cerium group from it. We shall await with interest the publication of his results.

URANINITE (pitchblende) occurs at the Deake, Lewis and Flat Rock mica mines in Mitchell County. Pure and unaltered masses of several pounds weight have been found. Cubes and which o-octahedrons imbedded in feldspar were obtained at the some Mine with a thin coating of uranotil or gummite. Some

* Geol. N. Car., Kerr, 1880, p. 84.

of the uraninite masses had a submetallic luster, quite like magnetite, and much of it was devoid of any pitchy appearance. *Gummite*,* *uranotil* and *uranochre*, occur at the above mines in considerable abundance; the three minerals are so intimately associated as to be inseparable, one specimen usually embraces them all. Pseudomorphs (cubes and octahedrons) after uraninite are quite common. A mass weighing six pounds six ounces, the largest yet discovered there, was found lately in the Flat Rock mine, which is partly unaltered uraninite. According to Dr. Genth,† this North Carolina gummite is a mixture of uranic hydrate, uranotil, lead-uranate and barium-uranate. Some of this North Carolina gummite is very beautiful; it varies in the same specimen from a bright lemon-yellow to deep orange-red and often has a core of velvet-black uraninite.

ÆSCHYNITE (?).—A mineral much resembling this species occurs in deeply striated prisms embedded in feldspar at Ray's mica mine. It is associated with apatite and beryl. It has not been analyzed. The crystals are large and well formed. Some groups of the crystals weigh a pound.

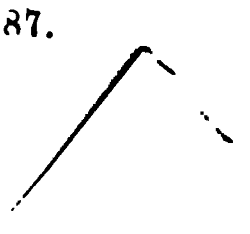
SAMARSKITE.—Another locality of this mineral has lately been discovered in Mitchell County. It can now be obtained in masses of many pounds weight. Hundreds of pounds are now awaiting purchasers. At the new deposit there is found associated with it a light brown, resinous-looking mineral of high specific gravity which may be massive hatchettolite, or a new species.

QUARTZ CRYSTALS *from Alexander County*.‡—Some interesting quartz crystals, found in Alexander County, are represented in the following figures. Among them, figures A, B, C and D, are examples of crystals terminated solely by planes in the 2-2 zone, which feature, as far as the writer can gather from the literature on the species, is new. Only in the counties of Iredell, Catawba, Alexander and Burke in North Carolina, and at Cumberland, R. I., have I found crystals having this interesting form. The series of planes *above* 2-2 are mostly rounded, but commonly have a good polish. They are invariably striated parallel to the edge of $2\text{-}2 / i$. Right and left-handed crystals are found. The crystal shown in fig. C is of interest since the edge between 3 and 3 is replaced by a plane, and since it has the dihexagonal prism $i\text{-}2$. Special attention is called to the *basal* truncation, fig. E, and to the plane between R and R in the $-\frac{1}{2}$ zone; also to certain inverted (depressed) triangular markings like those on crysta

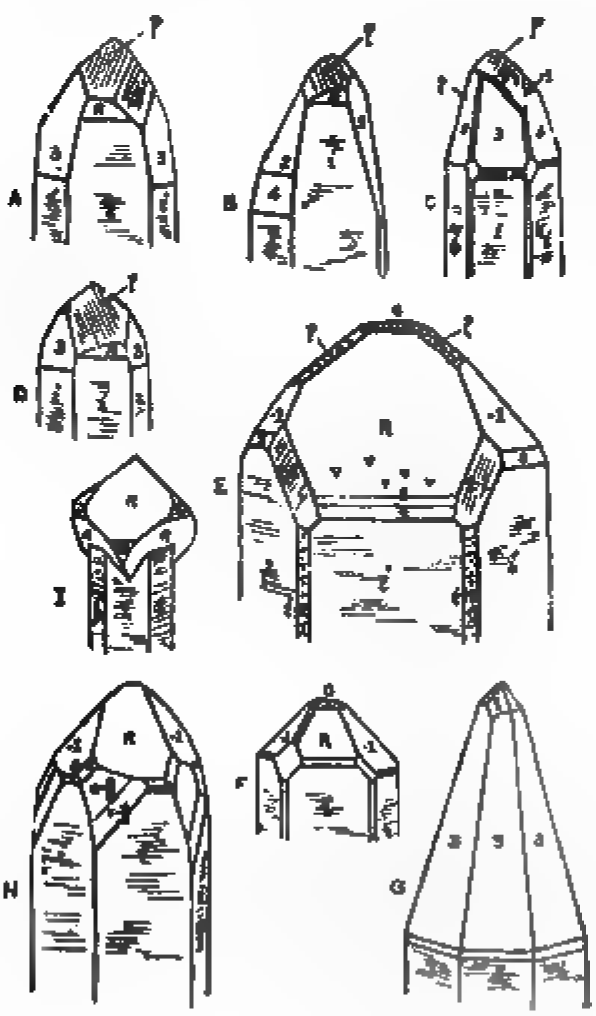
* Locality discovered by Prof. Kerr in 1877; see this Journal, xiv, 496.

† Geol. N. C., Kerr, 1880, page 34; also American Chemical Journa 1879.

‡ Geol. N. C., Kerr, 1880, page 87.



of diamond. The basal truncation and the (new?) plane in the $-\frac{1}{2}$ zone occur usually rough, though in two instances they were well polished planes. Fig. F has the dihexagonal pyramid in the i-2 zone. Fig. G represents a crystal almost wholly terminated by the rhombohedron 3. This plane is very common and largely developed on the Alexander County crystals. A fine example of this rare form is in the cabinet of Mr. J. A. Stephenson of Statesville, N. C. Fig. H shows a crystal having the planes 2-2, $3.\frac{3}{2}$,* and $4\text{-}\frac{4}{3}$* beveling every prismatic face at its intersection with R and -1. It also has other interesting rare planes. This crystal was perfectly pellucid, had a beautiful yellow tint and all its planes highly polished. Fig. I illustrates a form not uncommon in North Carolina. Often the cap or terminal crystal is strongly in contrast with the prism in color and transparency. Large groups are often found showing this second formation in parallel position.



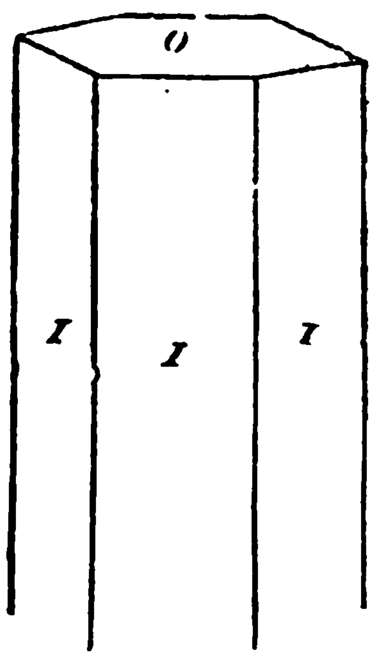
All the figures were drawn directly from the crystals and are of natural size: the determinations of the planes were made with an improvised goniometer and my lettering therefore may be only approximately correct.

BERYLS *from Alexander County.*†—Figs. 1 and 3 represent the extremes in form of these beryls. The crystal, from which fig. 3 was drawn, was at first mistaken for quartz. It was quite small, clear, had both ends terminated and with only a slight tint of green apparent. A crystal of this type but of more interest was collected by Mr. Stephenson from this same locality. It was terminated almost wholly by the planes $3\text{-}\frac{3}{2}$ and $4\text{-}\frac{4}{3}$. Fig. 2 is the most common form at the locality formerly known as the "Warren farm" They have been ‘ound there loose in the soil, of a light chrome green color, -ving prisms of six and twelve sides, and with polished ter- ıtions; the prismatic faces have a characteristic feature of whi. striated horizontally as if having been scratched with a some

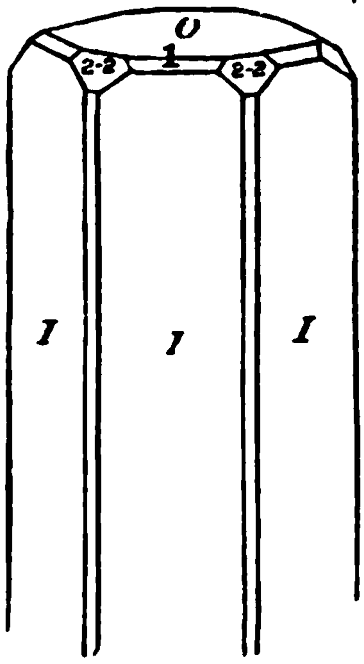
ore probably $6\text{-}\frac{6}{5}$ and $8\text{-}\frac{8}{7}$. † Geol. N. C., Kerr, 1880, page 88.

very coarse file. As yet they have not been found of sufficient depth of color and transparency for use as gems, but are quite unsurpassed by any beryls heretofore found in the United States. Those occurring in the soil have weathered out of cavities in the rock where they were formed. They were never imbedded, as some late work at the locality has well proven.

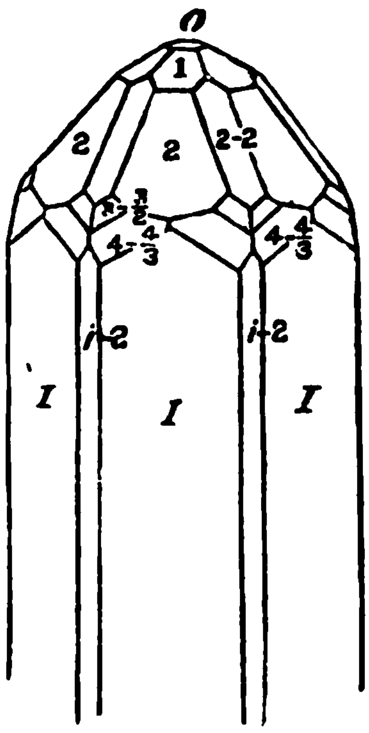
1.



2.



3.



Heretofore the only dependence for them has been the soil; now a narrow vein bearing them has been found by the writer and a shaft twenty-four feet deep has been sunk on it. It was the beautiful color of these beryls that prompted the work that so unexpectedly yielded the new variety of spodumene.* There are good indications of yet finding here the true beryl emerald, and it is with this end in view, coupled with the mining of the new spodumene emerald, that the writer is now at work in this State.

Platinum.—A diligent search for traces of this metal for five months in the auriferous regions of the Southern States in the interest of Mr. T. A. Edison resulted in finding *no traces of its existence.* The five reported localities in this State (N. C.) were carefully examined without success.

To the generous publicity that the late Professor Humphreys and Mr. J. Adlai Stephenson have given to their mineral researches in North Carolina, and to the sight of some of the many beautiful specimens they have sent north, the writer owes the impelling motive of his going to that State and the knowledge which has enabled him to succeed in his explorations.

Stony Point, N. C., Nov. 20th, 1880.

* This Journal, vol. xxi, Feb., 1881.

ART. IV.—*Variation in Length of a Zinc Bar at the same Temperature;* by Gen. C. B. COMSTOCK.

[Communicated by Authority of the Chief of Engineers, U. S. A.]

THE U. S. Lake Survey possesses a steel normal meter designated as (R. 1876), and a meter designated as (M. T. 1876), composed of a bar of steel and one of zinc so arranged as to form a metallic thermometer. Both were made by Repsold. It has also a base-measuring apparatus by Repsold of which the essential parts are tubes of cast iron four meters long, each containing in its interior a steel and a zinc bar arranged to form a metallic thermometer. Irregularities in the results of comparisons of two bars in the same tube, which were very marked functions of the temperature changes, led to an examination of the question whether a zinc bar has always the same length at a given temperature. The results seem to show conclusively that it has not. I have not met elsewhere with comparisons establishing such a change; if they have been made, these comparisons may give additional data. Mr. E. S. Wheeler, who made the larger part of the comparisons, first called my attention to the indications of a set shown by the ordinary comparisons.

As to the accuracy of the comparisons it may be said that they were made with an apparatus constructed by Repsold, in a comparing-room lined on all sides with saw-dust; that this lining reduces the diurnal temperature fluctuation to 0°·3 F.; that the changes in the external mean daily temperature rarely produce a change in the comparing-box exceeding 2°·5 F. per day; that but two visits were made to the comparing-room in a day; that the probable error in the result of one visit and comparison of two steel bars one meter long is about $1^{\mu}{\cdot}9$ (microns), and that artificial heat is not used. Temperatures were determined by thermometers whose probable errors do not exceed 0°·05 F., one lying on each meter.

In the experiments with the zinc bar of (M. T. 1876), this meter was alternately heated and cooled, and after each heating or cooling was compared with (R. 1876), which remained in the comparing-box during the twenty days covered by the experiments, its temperature varying in that time only about 3° F. In heating (M. T. 1876) it was taken from the comparing-room at a temperature of about 36° F. to another room, and kept at a temperature between 70° F. and 80° F. for twenty hours or more, then it was replaced in the comparing-box, where it cooled slowly to the temperature of the comparing-

room in about twenty-four hours. Comparisons with (R. 1876) were made during this period and for three days or more afterward. (M. T. 1876) was cooled from the temperature of the comparing-room to about $-3°$ F. by being placed for about twenty hours in a tin case surrounded by a mixture of snow and salt. Then it was placed in the comparing-box, allowed to approach the temperature of the comparing-room, and comparisons were made as before with (R. 1876). Temperatures of greatest cooling and heating were taken with maximum and minimum thermometers.

From comparisons at both high and low temperatures, the relative lengths and expansions of (R. 1876), (M. T. 1876) steel bar, and (M. T. 1876) zinc bar, are approximately known. They are, (R. 1876) = steel bar of (M. T. 1876)$+45^{\mu}\cdot7-0^{\mu}\cdot39\ (t-32°)$; zinc bar of (M. T. 1876)=steel bar of

$$(\text{M. T. } 1876)+267^{\mu}\cdot5+10^{\mu}\cdot15\ (t-32),$$

in which expressions t is the temperature in Fahrenheit degrees. The residual errors have been computed with these values. As the temperature-range was small during the comparisons given in the table, slight errors in expansion values will have little influence on the variations in the residuals.

In the following tables, the first column gives the date of comparison; the second and third give the temperatures of mercurial thermometers lying on the two meters; the fourth gives the residual errors of the comparisons of (R. 1876) and steel bar of (M. T. 1876) in the sense computed minus observed; and the fifth gives the residual errors of the comparisons of (R. 1876) and the zinc bar of (M. T. 1876). The section of these bars is 13^{mm} by 27^{mm}. In computing residuals the temperature of (M. T. 1876) is taken as the temperature of both meters.

From the residuals, considering only those comparisons forty-eight hours or more after the heating or cooling had ended, it is seen that the zinc bar of (M. T. 1876), when it is heated for twenty hours or more to a temperature of 70° F. and then is allowed to cool to its original temperature, 36° F., has a certain length; that if it is then cooled for twenty hours to a temperature of $-3°$ F., and afterwards is allowed to return gradually to its original temperature of 36° F., it will have a certain other length; and that these lengths at the same temperature may differ by fifteen microns. Both (R. 1876) and the bars of (M. T. 1876) were freely exposed to the air inside the comparing-box. If any large portion of the apparent change in length of the zinc bar of (M. T. 1876) was due to temperature errors, the residuals of the steel bars should show it at least in part.

Tables giving dates, temperatures and residuals of comparisons of (R. 1876) *and* (M. T. 1876) *made after periods of heating and cooling of* (M. T. 1876.) Preliminary reduction.

1. (M. T. 1876) heated, Feb. 7 to Feb. 14, 10.50 A. M. and kept at temperatures between 70° and 80° F.

Date of Comparison.	Temperature of (R. 1876.)	Temperature of (M. T. '76.)	(R. 1876)—(M. T. 1876)$_s$, computed, minus (R. 1876)—(M. T. 1876)$_s$, observed.	(M. T. 1876)$_r$—(M. T. 1876)$_s$, computed, minus (M. T. 1876)$_r$—(M. T. 1876)$_s$, observed.
1881.	°	°	μ	μ
Feb. 16, 9.14 A. M.	37·02 F.	36·91 F.	−0·4	−18·6
" 16, 8.19 P. M.	36·92	36·81	−1·7	−17·8
" 17, 9.12 A. M.	36·52	36·41	−0·4	−17·6
" 17, 7.58 P. M.	36·32	36·21	−0·4	−15·8
" 18, 9.35 A. M.	36·12	36·21	+0·3	−14·3
" 18, 8.49 P. M.	36·32	36·21	+1·2	−17·5
" 19, 9.25 A. M.	36·37	36·31	+3·0	−16·4
" 19, 8.05 P. M.	36·42	36·41	+0·5	−14·4
" 20, 10.38 A. M.	36·32	36·21	+1·7	−15·6
" 20, 8.37 P. M.	36·42	36·41	+1·3	−12·7
" 21, 9.56 A. M.	36·52	36·41	+3·2	−14·2
" 21, 8.09 P. M.	36·62	36·61	+1·0	−13·9
" 22, 10.12 A. M.	36·72	36·71	−1·2	−13·7
" 22, 8.44 P. M.	37·12	37·21	+0·9	−12·3
" 23, 9.22 A. M.	37·32	37·26	−0·9	−15·3
" 23, 7.35 P. M.	37·07	37·01	+1·1	−14·8
" 24, 9.17 A. M.	36·52	36·51	+1·4	−13·8

2. (M. T. 1876) cooled for 23 hours: Feb. 24, 10.00 A. M. to Feb. 25, 9.30 A. M. and kept at temperatures between −1° and −6° F.

	°	°	μ	μ
Feb. 25, 7.22 P. M.	35·52	35·42	+2·1	+2·4
" 26, 9.03 A. M.	34·91	34·82	+2·4	−0·5
" 26, 9.38 P. M.	34·91	34·82	+4·0	+1·6
" 27, 10.22 A. M.	35·31	35·22	+0·4	+0·3
" 27, 7.43 P. M.	35·91	35·81	−0·6	+1·7

3. (M. T. 1876) heated for 22 hours: Feb. 28, 11.30 A. M. to Mar. 1, 9.10 A. M., being kept at temperatures between 70° and 80° F.

	°	°	μ	μ
Mar. 2, 9.11 A. M.	37·02	36·96	0·0	−15·8
" 2, 9.04 P. M.	36·72	36·61	+0·3	−15·3
" 3, 9.07 A. M.	36·32	36·21	−0·2	−16·0
" 3, 8.51 P. M.	36·32	36·21	0·0	−15·7
" 4, 9.09 A. M.	36·32	36·21	−2·4	−14·3

4. (M. T. 1876) cooled for 24 hours: Mar. 3, 9.30 A. M. to Mar. 4, 9.30 A. M., being kept at temperatures between −2° and −5° F.

	°	°	μ	μ
Mar. 5, 8.58 P. M.	36·72	36·61	+2·6	+6·6
" 6, 9.50 A. M.	36·82	36·71	+2·2	+6·4
" 6, 8.04 P. M.	37·22	37·16	+1·1	+6·0
" 7, 8.52 A. M.	37·32	37·21	+1·4	+5·1
" 7, 7.56 P. M.	37·58	37·51	+2·8	+5·3
" 8, 9.03 A. M.	37·88	37·81	+3·0	+5·3

(M. T. 1876)$_s$ denotes the steel bar of (M. T. 1876) and (M. T. 1876)$_z$, the zinc bar μ is the symbol for micron or thousandth of a millimeter.

The tubes of the Repsold base-apparatus have already been spoken of. A similar experiment was tried with these tubes.

The zinc bars of tube No. 1 and of tube No. 2, as well as their steel bars, were first compared with each other at about 41° F.; then tube 1 was heated for twenty-four hours to a temperature between 70° and 80°, and after the heating the two zinc and the two steel bars were again compared. The relative lengths and expansions of the two steel and of the two zinc bars are given approximately by

$$S'_1 = S'_2 + 1518^{\mu}\cdot 8 - 0^{\mu}\cdot 06t,$$
$$Z'_1 = Z'_2 + 210^{\mu}\cdot 6 - 0^{\mu}\cdot 44t,$$

where t is the temperature of the comparison in Fahrenheit degrees. The lengths designated by S'_1, S'_2, Z'_1, Z'_2, are each very nearly four meters, but are not the lengths used in base measurement. The former are in the neutral axes of the bars and have been used to avoid any question of lateral flexure. Temperatures were observed with three well determined thermometers in the interior of each tube.

In the following table it is assumed that the observed mercurial temperatures are the true temperatures of the bars. The absolute expansions of the bars are known, and with them the observed difference of length of the two bars is reduced to what it would have been if the two bars under comparison had had the same temperature. This is called the observed difference of length of the two bars. Subtracting it from the difference of lengths of the two bars at that temperature as computed from the equations given above, the residuals result. When positive, they indicate that the observed difference of length of the two bars was algebraically too small.

The first column gives the date of the comparison; the second and third, the mercurial temperatures of tube 1 and tube 2; the fourth, the residuals of the steel bars or $S'_1 - S'_2$ computed, minus $S'_1 - S'_2$ observed; the fifth, the residuals for the zinc bars or $Z'_1 - Z'_2$ computed, minus $Z'_1 - Z'_2$ observed.

PRELIMINARY REDUCTION.

Date.	t_1.	t_2.	$S'_1-S'_2$ residuals.	$Z'_1-Z'_2$ residuals.
1881.	°	°	μ	μ
Mar. 14, 9.40 A. M.	39·74 F.	39·82 F.	−1·0	−2·3
" 15, 9.39 P. M.	40·58	40·67	−3·8	−5·2
" 16, 3·36 P. M.	41·79	41·80	−8·9	−2·5
" 17, 9.27 A. M.	41·90	41·90	−9·7	—2·4

Tube 1 from Mar. 17, 9.30 A. M. to Mar. 18, 9.15 A. M., was kept at a temperature between 70° and 80° F.

	°	°	μ	μ
Mar. 18, 8.15 P. M.	46·51	45·12	−10·1	−58·9
" 19, 9.44 A. M.	44·25	43·69	− 8·4	−46·8
" 19, 2.32 P. M.	44·02	43·52	+ 0·9	−38·8
" 19, 8.08 P. M.	43·88	43·50	+ 2·7	−42·1
" 20, 9.39 A. M.	43·66	43·37	+ 4·6	−33·5
" 20, 8.35 P. M.	43·70	43·45	— 6·4	−38·8
" 21, 10.23 A. M.	43·47	43·33	+ 3·5	−35·3
" 21, 8.17 P. M.	43·51	43·32	+ 1·1	−33·4
" 22, 9.12 A. M.	43·33	43·12	+ 2·6	−28·8
" 22, 8.43 P. M.	43·03	42·90	− 4·9	−26·4
" 23, 9.18 A. M.	42·76	42·59	— 0·6	−29·7

An examination of the residuals shows that the mean residual of $S'_1-S'_2$ before heating was $-5^{\mu}{\cdot}8$, and allowing forty-eight hours to cool, that the mean residual from 9^{h} 39^{m} A. M., March 20, to 9^{h} 18^{m} A. M., March 23, was $0^{\mu}{\cdot}0$, differing $5^{\mu}{\cdot}8$ from the previous value, a quantity too small, in view of the very large residuals before heating, to indicate a change in $S'_1-S'_2$. But the mean residual of $Z'_1-Z'_2$ before heating was $-3^{\mu}{\cdot}1$, and after heating, between March 20, A. M. and March 23, was $-32^{\mu}{\cdot}2$, a change of 29^{μ}.

It seems, then, that the heating from 41° F. to 75° F., and subsequent cooling to 43° F., increased the length of the four-meter zinc bar about 29^{μ}. This would give a change of 7^{μ} per meter for a temperature change of 30°, or about half the change found for the zinc bar of the meter (M. T. 1876) for a temperature change from −3° to +75°.

Sufficient data have not yet been obtained to determine the time required for a zinc bar to lose this probably temporary change of length. In the case of glass thermometers it is known that sub-permanent changes of form lasting for many weeks occur on heating them.

The question at once occurs, whether bars of other metals may have sensibly differing lengths at the same temperature.

U. S. Lake Survey Office, Detroit, Mich., April 30, 1881.

ART. V.—*Restoration of* DINOCERAS MIRABILE; by Professor O. C. MARSH. With Plate II.

THE order of extinct gigantic mammals discovered by the writer in 1870, in the middle Eocene of Wyoming, and named *Dinocerata*, has now been investigated, and all the more important characters of the skeleton carefully determined. In this peculiar group of Ungulates, there are three well-marked genera: *Dinoceras* Marsh, the type genus, *Tinoceras* Marsh, and *Uintatherium* Leidy. These will be fully described by the writer in an illustrated monograph now nearly ready for publication. This memoir will be based upon the remains of more than one hundred and fifty distinct individuals of this order, now deposited in the Museum of Yale College.

The type species of the *Dinocerata* is *Dinoceras mirabile* Marsh, and especial pains have been taken to work out the osteology of this animal, as a key to the structure of the group. Almost every bone in the skeleton is now known by various specimens, and this affords ample material for a restoration which will represent very nearly the osseous framework of the animal when alive. Such a restoration has been attempted for the memoir in preparation, and in the present article a much reduced figure of this is given (Plate II), which shows the general proportions of the type species.

Among the points of special interest suggested by the restoration of *Dinoceras* here presented are the following:

(1.) *The absence of a proboscis.* There is no evidence in the skull of the existence of such an organ, and the proportions of the neck and fore limbs certainly rendered its presence unnecessary.

(2.) *The "horn-cores" of the skull.* An examination of a large number of these, from individuals of various ages, indicates that the posterior pair, on the parietals, were sheathed with thickened integument, which may have developed into true horn, as in the Pronghorn (*Antilocapra Americana*). The surface of the osseous protuberances is very similar in both cases. The pair of elevations on the maxillaries are equally rugose, and bear evidence of a similar covering. The small tubercles on the nasals are usually smoother, and were probably without horn-like sheathing. The three pairs of elevations are present in both sexes, but are proportionally smaller in the females.

(3.) *The canine tusks*, also, are common to both sexes, but those of the males only are large and powerful.

(4.) *The dependant processes of the lower jaw* correspond in size with the canine tusks, and are evidently adapted for their protection. In the females, these processes are but slightly developed.

(5.) *The sternum* is composed of flat horizontal segments, not compressed and vertical, as in Perissodactyls.

The material now available for a restoration of *Tinoceras grande* Marsh, is sufficient to show that this animal was similar in general proportions to *Dinoceras mirabile*, but of much larger size. The few specimens that can at present be referred to *Uintatherium* leave many points in its structure undecided. The type specimen of this genus is from a lower horizon than that of either *Dinoceras* or *Tinoceras;* and the evidence now at hand seems to indicate that *Uintatherium* is the oldest and most generalized form of the *Dinocerata*. One specimen in the Yale Museum from near the original locality, and agreeing, so far as the comparison can be made, with the type, has four lower premolars. This character will serve to distinguish *Uintatherium* from *Dinoceras*, to which it has various points of resemblance. *Tinoceras* is from a horizon higher than *Dinoceras*, and is much the most specialized genus of the group.

Yale College, New Haven, June 14th, 1881.

ART. VI.—*On the Torbanite or "Kerosene Shale" of New South Wales;* by A. LIVERSIDGE.*

The so-called "kerosene shale" does not differ very widely from cannel coal and torbanite. Like cannel coal, it usually appears to occur with ordinary coal in the form of lenticular deposits. Like cannel coal also, when of good quality it burns readily without melting, and emits a luminous smoky flame. When heated in a tube it neither decrepitates nor fuses, but a mixture of gaseous and liquid hydro-carbons distils over.

In color it varies from a brown-black, at times with a greenish shade, to full black. The luster varies from resinous to dull. When struck it emits a dull wooden sound. The powder is light brown to gray; the streak shining.

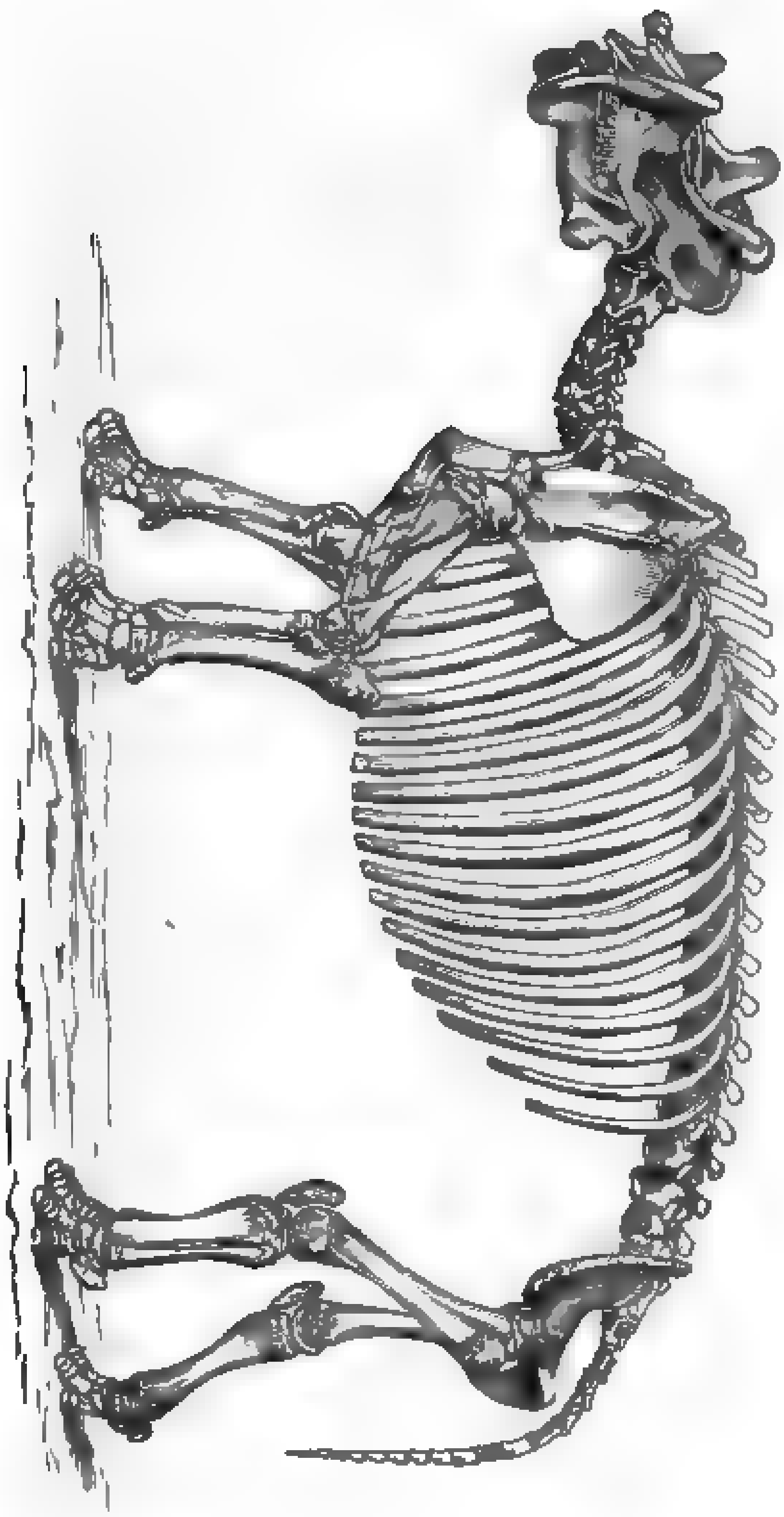
Professor Silliman proposed the name of *Wollongongite* for the mineral; but this has not come into general use, neither is it an appropriate name, since the specimen sent to him was not from Wollongong, but from Hartley.

Analyses afforded:—1, 2, 3, From Joadja Creek, color black, brownish, sp. gr. 1·103, 1·054 and 1·229; 4, From Murrusundi, dark-gray, but with white clayey specks.

Loss at 100° C.	1·160	·440	·040	1·165
Volatile hydro-carbons	73·364	83·861	82·123	71·882
Fixed carbon	15·765	8·035	7·160	6·467
Ash	9·175	7·075	10·340	19·936
Sulphur	·536	·589	·337	·549

A specimen from the Hartley seam, where most free from mineral matter, having sp. gr. 1·052, afforded: Moisture and volatile hydro-carbons 82·24, fixed carbon 4·97, ash 12·79=100. An ultimate analysis of the same, dried at 100° C., gave: Carbon 69·484, hydrogen 11·370, oxygen, nitrogen, and sulphur 6·356, ash 12·790 =100.

* *Abstract* from paper in Proc. Roy. Soc. N. S. Wales, Dec., 1880.



Restoration of DINOCERAS MIRABILE, Marsh; one twenty-fifth natural size.

Art. VII.—*Meteorological Researches, Part II. Cyclones, Tornadoes and Waterspouts*; by Wm. Ferrel.*

[Abstract, published by permission of Carlile P. Patterson, Superintendent of the United States Coast and Geodetic Survey.]

If all parts of the atmosphere had the same temperature and the same hygrometric state it would remain in a state of static equilibrium. The principal circumstance which disturbs this equilibrium is the difference of temperature between the equatorial and polar regions. This gives rise to an interchanging motion of the air, toward the equator below and from it above, and if it were not for the effect of the earth's rotation on its axis this interchanging motion would be at all places in the direction of the meridian, and would be continually accelerated in its initial motions, until the friction arising from these motions would exactly equal the force producing them, after which the motions of any one place would be constant, but of course different at different places. The now well-known effect of the earth's rotation is to give rise to a deflecting force to the right of the direction of the moving body in the northern hemisphere and the contrary in the southern, whatever may be the direction of motion. Hence the air in moving above toward the poles, is deflected toward the east and in moving toward the equator below, toward the west, so that the tendency is for the air to assume an eastward motion in the middle and higher latitudes, and a westward motion nearer the equator. These latter motions combined with the interchanging motions between the equatorial and polar regions give rise to what are called the general motions of the atmosphere, depending upon the difference of temperature between these regions and independent of local disturbances of temperature.

The amount of eastward motion depends upon the amount of friction, and must be such that the friction at the earth's surface is equal to the force causing this component of motion, and the same with regard to the westward motions. According to well established principles of mechanics, there cannot arise any force from the effect of the earth's rotation, which by means of friction would tend to either increase or decrease the earth's rotation, and hence the eastward and westward components of motion must be so adjusted that the sum of all the moments of force acting upon the earth through friction and tending to affect its rotation, must be equal 0, and hence, as there are eastward components of motion in the higher latitude, there must necessarily be westward ones nearer the equator. The

* Coast and Geodetic Survey Report for 1878. Appendix 10.

eastward motions in the higher latitudes increase with increase of altitude, but nearer the equator the westward motions decrease with increase of altitude and at a certain altitude vanish and become eastward motions.

The deflecting force depending upon the earth's rotation is such that if the air on the parallel of 45° has a velocity of 54 miles per hour, it gives rise to a gradient of pressure, increasing to the right of the direction of motions in the northern hemisphere, and the contrary in the southern, of 0·1 inch of mercury in the distance of one degree of a great circle of the earth. This force, and consequently the gradient, is as the velocity and the sine of the latitude, and hence it is a maximum at the pole and decreases toward and vanishes at the equator. The eastward motion, therefore, in the middle and higher latitudes gives rise to a gradient of pressure increasing *toward* the equator, and the westward motion between the tropics and the equator to a gradient of pressure increasing in a direction *from* the equator, and hence there must be a belt of higher pressure all around the globe, having its maximum at the latitude of 30° or 35°; where the dividing line is between the eastward and westward motions. The pressure diminishes from this maximum toward the poles, so that the pressure at the poles, especially the south pole, is less than at the equator. As the southern hemisphere is mostly covered by the ocean, on which the friction is much less than on land, the eastward velocities in the middle and lower latitudes of this hemisphere in their normal state, amount to almost a gale entirely around the globe, and these give rise to a very steep gradient there, and a great barometric depression at the south pole.

The regularity of the general motions of the atmosphere and of the gradients depending upon them, is very much interfered with by irregularities in the distribution of the earth's temperature arising from ocean currents, and from irregularities of the earth's surface, comprising both sea and land with its mountain ranges. This part of the subject was treated in Part I, of these researches, but some knowledge of the principles contained in this part of the subject and of the results is necessary to understand the theory of cyclones, tornadoes, etc.

Cyclones.—Cyclones arise from more local disturbances of temperature. On account of the want of homogeneity of the earth's surface and of the hygrometric state of the atmosphere, the amount of heat received and radiated by the earth's surface and the atmosphere, is very different in different localities. Where more heat is received than radiated, the temperature must continue to rise until the loss of heat by radiation and other means exactly equals the amount received, and hence there cannot be uniformity of temperature even on the same

latitudes, and there must be a great many local irregularities in the distribution of temperature independent of the great general disturbance of the equality of temperature between the equatorial and polar regions. These must give rise to corresponding motions of the atmosphere which are superadded to those of the general motions. If in the unequal distribution of temperature it should happen, as it must frequently, that there is a somewhat circular area with higher temperature in the interior and with temperature gradients increasing somewhat regularly on all sides from the center outward, we should have, at least approximately, the initial condition of a cyclone. There would be a motion of the air from all sides toward the central part of the warmer and more rare air in the interior, a very slow rising up of the air in this part and a flowing out of the air above; that is, there would be an interchanging motion between the colder and warmer parts of the air, just as in the case of the general motions of the atmosphere there is between the equatorial and polar regions, except that in the one case the flow is toward the central part below and from it above, while in the other it is the reverse. Any limited portion of the earth's surface of not very great extent, may be regarded as a plane, and this by virtue of the earth's rotation, has a gyratory motion around its center, equal to that of the earth's rotation multiplied into the sine of the latitude of this center. Hence, as in the case of the general motions of the earth, this interchanging motion between the central and exterior part of the warmer and more rarified air, must give rise to gyrations around the center from right to left in the northern hemisphere, with gyrations the contrary way in the exterior part, and these gyrations in contrary directions must give rise to gradients of pressure increasing in the central part from the center outward, but in the external part to a gradient of pressure increasing from the outward limit of the gyrations toward the center, so that there must be a belt of high pressure with its maximum where the interior gyrations in proceeding from the center, vanish and change signs. These exterior gyrations and the gradients arising from them are generally small in comparison with those of the interior, and they are generally so interfered with by numerous irregularities, that they are not readily shown by observation, but to deny that they exist, would be to deny the truth of a fundamental and well established principle in mechanics.

The increased pressure under the belt of high barometer surrounding the central part of the cyclone causes a modification of the flow of air toward the center very near the surface, for the air is forced out from beneath in both directions, the flow toward the outward border very near the surface counter-

acts and reverses the flow toward the center arising from the primary and initial cause of disturbance, while the part pressed out on the interior side toward the center, combines with this flow toward the center and increases it. For the same reason in the general motions of the atmosphere the flow of air below from the polar to the equatorial regions is reversed very near the surface, and the gentle southwest winds of the middle latitudes are produced.

The preceding condition, found in the unequal distribution of temperature, must be regarded simply as a primary cause of disturbance, giving rise merely to the initial cyclonic disturbances; for without other conditions, depending upon the hygrometric state of the atmosphere, and upon the rate of decrease of temperature with increase of altitude in the atmosphere generally in which the cyclone exists, we could have no cyclone of long continuance or of much violence. With a dry atmosphere the air in the ascending current of the interior would cool about one degree centigrade for each 100 meters of ascent, so that the air at a very moderate elevation would become colder and more dense than that of the strata of the surrounding atmosphere at the same altitude. The pressure then of the air at the surface in the interior would become equal to or greater than that of the air generally, unless the rate of decrease of temperature with increase of altitude in the latter were greater than 1° C. for 100 meters, which it never is except in some rare cases and very near the earth's surface only. When this would take place the initial cyclonic disturbances arising from this primary cause of disturbance would cease.

If the air is nearly saturated with aqueous vapor, after ascending to only a moderate elevation its tension and temperature are so much diminished that the vapor is condensed into cloud and rain and the heat given out in the condensation of the vapor as the air ascends prevents the rapid cooling which takes place in dry air and the rate of cooling with increase of altitude is reduced, in ordinary temperatures and elevations, to less than half of what it is in dry air. If in this case the rate of decrease of temperature with increase of altitude in the surrounding atmosphere generally is less than that in an ascending current of saturated air, then the temperature of the air in the ascending current, at all altitudes, must be less than that of the air generally, and hence the column of ascending air is lighter than the surrounding air, and the ascending current is kept up as long as it is supplied with air nearly saturated. If, however, after a time, this current comes to be supplied with dryer air, then it has to ascend to a much greater elevation before condensation of the vapor takes place, and as

it cools at the rate of 1° C. for each 100 meters before it reaches that elevation, it may be cooled down lower than the surrounding air before reaching the elevation where condensation commences, so that if, in this case, we should have the conditions of a continuing cyclone at all, the power of the cyclone would at least be very weak.

Where the state of the atmosphere is such, whether dry or saturated with moisture, that the rate of decrease of temperature with increase of altitude is greater than in an ascending current, it is said to be in a state of unstable equilibrium, since if from any slight predisposing cause such ascending current is once set in motion it must continue until this state is changed, either by the action of what we have called the primary causes of disturbance of temperature or from the inverting action of the currents set in motion. But an atmosphere in this state over a large area would not furnish the conditions for a large cyclone, but there would be simply a bursting up of the lower strata through the upper ones at various places, giving rise to numerous local showers, and often to tornadoes and hailstorms. In order to have the complete conditions of a large cyclone it would be necessary to have a central region of warmer and more rarefied air to set in motion ascending currents over a considerable area, and with this there might be considerable cyclonic disturbance if the atmosphere were not quite in the state of unstable equilibrium, but without this latter condition also we could not have a long continued cyclone. It is seen then that the moisture of the air is a very important element, since without this we cannot have the state of unstable equilibrium unless the rate of decrease of temperature with increase of elevation in the atmosphere generally is greater than 1° C. for each 100 meters, but where the air is saturated this condition takes place with a rate of decrease less than half as great, a rate of decrease which is often found in the atmosphere. The more nearly the air is saturated with vapor, and the greater the decrease of temperature of the air generally with the increase of elevation, the greater is the power of the cyclone. But without these there may be considerable cyclonic disturbance kept up for some time, arising from the primary causes of disturbance, even where the air is so dry that there is very little condensation of vapor into cloud and rain. Professor Loomis has shown that there is sometimes a considerable barometric depression for several days with little or no rain, but in such cases there are only small gradients with no violent winds, and the depression only becomes considerable from the gradients extending over a large area. At the equator where there is no gyration of the area of rarefaction around its center in virtue of the earth's rotation

around its axis there cannot be any gyratory motion, but the interchanging motion between the central and external part is entirely radial. Cyclones are therefore never observed on or very near the equator.

If there were no friction between the air and the earth's surface, all the conditions of a cyclone could be satisfied by circular gyrations without any radial motions, except in the initial state before the radial motions are brought to rest by means of the friction between the different strata. In this case the linear velocity of the gyrations would be very great near the center. The greater the amount of friction between the air and the earth's surface the less is the velocity of these gyrations, and the greater the inclination of the direction of motion at the earth's surface from the direction of the tangent toward the center. This is shown by the mathematical expression of this inclination deduced from the solution of the equations expressing the conditions of a cyclone, and this same expression shows that near the center of a cyclone the gyrations at the surface are more nearly circular than at greater distances from it, and that, all other circumstances remaining the same, the nearer the equator the greater the inclination, so that at the equator it becomes 90°, and the motion, as already stated, is radial. In the exterior, or anticyclonal part, where the gyrations are reversed, this inclination at the earth's surface is outward from the tangent. At all altitudes some distance above the earth's surface the friction is small and the gyrations are more nearly circular, but a little inclined toward the center in the lower part where the interchanging motion is toward the center, but outward from the center above, where this motion is from the center.

If any central area for some reason could be kept colder than the surrounding parts, with a gradient of temperature increasing somewhat regularly from the center outward, we should have the condition of a cyclone with a cold center. This condition is furnished in some measure by an island in a northern sea in winter, on which the temperature is less than on the surrounding ocean. In such a case the interchanging motions below and above would be reversed, but the gyrations would be in the same direction around the center in the interior part as in the case of an ordinary cyclone, and the contrary in the exterior part. The general motions of the atmosphere on each hemisphere of the globe, with the cold poles as their centers, are simply two examples of cyclones of this sort. The gyrations here, in the northern hemisphere, are around the pole from right to left, as in an ordinary cyclone, and the contrary in the southern hemisphere, while at a certain distance from the center, or pole, these gyrations

vanish and change signs, then giving rise to the anticyclonal part of the system, as in an ordinary cyclonic system.

A local cyclone of this sort, with much violence or long continuance, cannot take place. For if there was a central colder area which would give rise to the initial motions of such a cyclone, the air in its descent in the interior would become 1° C. warmer for each one hundred meters of descent, and hence the colder initial temperature of the central part would soon be so increased as to equal that of the atmosphere generally surrounding, when the condition giving rise to initial motion would be destroyed and all motion cease. In such a case there would be no advantage in a moist atmosphere, since if it were even saturated as soon as descent in the interior would commence, it would become unsaturated. Hence we never have any violent cyclones of this sort, and nothing more than initial disturbances which continue generally only a short time.

Fixed Cyclones.—Where the primary cause of temperature disturbance is fixed to one spot on the earth and kept up continuously, it gives rise to a fixed cyclone. Such an example is furnished by a warm island surrounded by a colder sea. This, unless it were very near the equator, would give rise to considerable cyclonic disturbance, and, if the island were of considerable extent, to an observable barometric depression. A very remarkable example of such a cyclone exists in the northern part of the Atlantic ocean. Here, on account of the Gulf Stream and the general interchange of waters between the equatorial and polar regions, which tend to equalize the temperatures, there is a considerable area of warmer temperature, especially in the winter season, than that of the sorrounding parts, with its center near Iceland. This gives rise to a fixed cyclone with its interior gyrations around this center and fixed area of low barometer extending over the greater part of the northern part of the Atlantic ocean. These gyrations on the southern side of this cyclone, combining with those of the general motions of the atmosphere, cause the strong west winds and steep gradients in the middle latitudes of this ocean in the winter. The belt of high pressure of this cyclone is thrown somewhat, on the south side, upon that due to the general motions of the atmosphere at the parallel of about 30° or 35°, and causes the area of high pressure in this ocean at these latitudes.

In the summer season the temperature gradients nearly disappear, and there is very little cyclonic disturbance over this region or barometric depression in the vicinity of Iceland. Very similar conditions exist in the northern part of the Pacific ocean, but the cyclonic disturbances and the decrease of barometric pressure are not so great.

Progressive motions of Cyclones.—Ordinary cyclones, at least soon after their first formation, become independent of local circumstances connected with the earth's surface. The primary temperature disturbance is not sufficiently great and permanent enough to hold the cyclone to the spot where it originates, and it is carried forward by the prevailing general movements of the atmosphere, and the central area of warmer air is maintained by the heat arising from the condensation of the vapor in the interior ascending currents supplied with moist air from the earth's surface by means of the horizontal currents flowing in from all sides toward the center. The direction of progressive motion, therefore, is somewhat in the direction of the general motions of the atmosphere in all parts of the earth. Hence cyclones originating near the equator, where there is a westward component of motion, are carried westward, but those originating in the middle latitudes, where the general motion of the atmosphere is eastward, are carried toward the east. There is also a tendency of cyclones to move toward the poles where there are no general currents to carry them forward. Cyclones, therefore, which originate in the Atlantic near the equator are first carried westward and northward toward the West India islands, and Florida, until they arrive at the parallel of about 30°, where there is no east or west component of motion, and where, consequently, they move in the direction of the meridian until they arrive at the middle and higher latitudes, where the general eastward current carries them in that direction, with an inclination still toward the pole. This seems to be the general tendency of cyclones originating everywhere near the equator, but they seem to make their way through toward the pole with greatest facility on the west sides of the Atlantic and Pacific oceans, because there the general motions of the air are deflected around somewhat toward the pole, and aid the cyclones in their progress and carry along a supply of moist air from the equatorial regions for their support. As the power of the cyclone is mostly in the upper cloud region of the atmosphere where the vapor is condensed mostly, the progressive motions of the cyclones depend rather upon the general motions of the atmosphere at considerable altitudes than upon those near the earth's surface. Hence within the tropics, where the westward motion is small above, the progressive velocity of the cyclone is small, and it is so at the vertex of the parabolic path where the motion is toward the pole, but after arriving at the higher latitudes where the upper general motion of the atmosphere has considerable velocity, the progressive motion of the cyclone is much accelerated, especially its eastward component.

It must not be supposed, however, that the progressive motion of cyclones depends entirely upon that of the air in which

the cyclone exists. It depends also very much upon the direction in which the greatest humidity of the air lies. The progressive motion of the cyclone is generally greater than that of the air, even in the upper regions, and consists rather in the continual formation of new cyclones a little in advance of the old ones, the latter gradually subsiding, and this new formation is mostly likely to occur in the direction of greatest moisture.

Areas of High Barometer.—These arise from the intersecting and overlapping of the circular belts of high barometer of different cyclones both fixed and progressive. In consequence of the gradients arising from the general motions of the atmosphere combined with those of the fixed cyclones and all the other irregularities, the gradients and isobars become very irregular. When to these are added the irregularities of progressive cyclones following and impinging upon one another, this irregularity becomes still much greater, so that it must frequently happen that there are areas in which the barometer stands higher than at any of the surrounding places, just as on a rough sea where numerous broad waves interfere and cross one another, the surface of the sea has elevations and depressions, not in the form of waves and troughs, but rather of elevated and depressed areas approximating more nearly to a circular form. The isobars of these areas are generally somewhat irregular, but still as they enclose an area, and the winds, according to a well-established law, must blow with a certain not very great inclination to these isobars, the motion of the air is somewhat around these areas in a direction contrary to that of the interior part of an ordinary cyclone. These areas, however, do not form systems of winds complete in themselves, but simply arise from the interference of cyclones, and are therefore not properly called anti-cyclones.

Tornadoes.—These are simply very small cyclones, extending over so small an area that the effect of the earth's rotation has no sensible influence, and the gyrations arise, not from the gyration of this small area around its center in consequence of the earth's rotation, but from a disturbed state of the atmosphere in which the tornado occurs which renders it impossible for the air to flow from all sides toward a center without running into gyrations around that center. This may be illustrated by means of a basin of water with a hole through the bottom in the center through which the water is allowed to run out. If the water is entirely at rest when the flow commences, there will be only a radial and very gentle motion of the water from all sides toward the center, without any gyratory motion, but if it has the least gyratory motion in its initial state, even entirely imperceptible, it will run into very rapid gyrations before reaching the center.

The effect of friction in tornadoes is much less than in cyclones. A cyclone of considerable extent may be regarded as a disk, with a diameter many times greater than its depth or thickness, and hence the gyrations are very much retarded by friction on the earth's surface; but a tornado is rather a pillar of gyrating air with a very small base in comparison with its altitude, and hence the retardation of the gyrations by friction on the earth's surface in this case is comparatively very small. The gyration of the air, therefore, except near the earth's surface, is very nearly in accordance with the principle of the preservation of areas, and hence the lineal gyratory velocity is very nearly inversely as the distance from the center, and consequently must become very great near the center.

In cyclones the barometric gradient and depression of the barometer in the central part are due both to the deflecting force arising from the earth's rotation and the centrifugal force of the gyrations, to the former mostly at a considerable distance from the center, but to the latter mostly near the center. In a tornado the diminution of pressure and tension in the center arises almost entirely from the centrifugal force, that depending upon the earth's rotation being nearly insensible. On account of the rapidity of the gyrations near the center this diminution of pressure may be very great there, while at a very short distance from the center it is imperceptible.

Tornadoes occur when, from any cause, the air is in the state of unstable equilibrium already referred to. This may be near the earth's surface, but is most usually up in the region of the clouds, where the air is saturated with moisture, and where consequently this state occurs most frequently, since it then requires a rate of diminution of temperature with increase of altitude usually less than half as great as in the case of dry air. When the atmosphere is in this state the air of the lower strata, from any slight disturbance, bursts up through the upper strata at some point, and the higher it ascends the greater is the difference between its temperature and density and those of the surrounding strata at the same elevation, and hence the greater the tendency to rush up at that point. But, as in the case of the basin of water, if the initial state of the air were that of quiescence, there would be only a radial flow of air from all sides toward that point without any gyratory motion or diminution of tension at the center, and with very little violence of motion. The velocity of the ascending current in this case would not be very great since the column of ascending air would soon spread out laterally, and become too great. In order to have, therefore, all the conditions of a tornado, it is necessary to have, besides the state of unstable equilibrium, the other conditions which, as in the case of the water in the

basin, give rise to gyrations around the central point toward which the air from all sides flows. When these gyrations commence above, as they usually do, since the air there is most frequently in the state of unstable equilibrium, they gradually extend downward for the gyrations cause a great diminution of tension and of density, and the air consequently in the center rushes up with great velocity and that below of the still unagitated strata is drawn in to supply its place, which likewise runs into gyrations around the center, so that the gyrations in a very short time extend down to the earth's surface. The whole column of gyrating air is like a tall flue containing very rarefied air, the centrifugal force of the gyrations acting as a barrier to prevent the inflow of air from all sides into the interior, and if the gyrations at the earth's surface were as rapid as those above, it would be similar to such a flue with all the draught cut off. But very near the earth's surface these gyrations, and consequently the centrifugal force, are very much diminished on account of the friction at the surface, and this allows the air to rush in quite near the surface to supply the draught of the interior ascending current. While, therefore, the gyrations above, on account of the little friction are almost exactly circular, allowing little air to reach the central part, the motion of the air, near the surface, is more nearly radial, or at least very much inclined inward from the direction of the tangent. It is the same somewhat in the case of large cyclones. Very near the earth's surface the radial component of motion is much greater than it is at a moderate elevation above, and the inclination from the tangent toward the center may be very great, while a little above the surface the gyrations are nearly circular. It is readily seen that this must be the case since the force which overcomes the friction of the gyratory motion depends, in both cyclones and tornadoes, upon the radial component of motion, and hence the greater the friction to be overcome the greater must be this radial component, and where there is little friction this radial component is very small and the gyrations nearly circular.

Where the air near the earth's surface is nearly saturated with moisture it has to ascend to only a very moderate altitude, at the outer border of the tornado, to have its tension and temperature so reduced that the vapor is condensed into cloud, and nearer the center, where the tension is diminished by the centrifugal force of the gyrations, the stratum in which condensation and cloud-formation commences is brought down to the earth at a considerable distance from the center. In such a case a considerable area of the earth's surface in the central part of the tornado is covered with dense cloud and enveloped in darkness. The indrawing, gyratory and ascensional currents

are so strong as to draw in and carry up very heavy bodies and throw them out above to a great distance. Sometimes the ascending current is so strong as to keep a heavy body suspended in the air for a long time until the tornado has progressed many miles, when, after the violence of the tornado begins to abate, the body falls to the earth. Unless the strength of the ascending current is sufficient to carry the body up to an altitude where the air tends outward from the center, the gradually indrawing currents below that altitude keep the body near the center and it cannot fall to the earth until the ascending velocity of the current which has carried it up, is diminished.

Waterspouts.—These are simply special cases of tornadoes, as tornadoes are of cyclones. Where the air at the earth's surface in a tornado is not nearly saturated with moisture, it has to ascend to a much greater elevation on the outward border of the tornado before cloud-formation takes place, and also the nearly horizontal inflowing and gyratory currents below have to approach very near the center before cloud is formed, and the nearer the earth's surface, the nearer this approach must be. Hence, the base of the cloud assumes a funnel-shape above, with a long tapering stem reaching down to the earth or sea. A waterspout, therefore, is simply *the cloud brought down to the earth's surface by the rapid gyratory motions near the center of a tornado.* This may be explained by means of a deep vessel, instead of a shallow basin, of water with a hole in the center of the bottom. If the water is allowed to run out, and it has only an almost perceptible initial gyratory motion, it finally runs into very rapid gyrations around the center, and the surface of the water and each of the strata of equal pressure under the surface, assume a funnel shape at the top and extend down to the bottom, even within the hole, in the form of a long, tapering tube. It is the same in the case of the air in a tornado. The fact that the air of the lower strata runs upward through the upper strata, instead of downward through the bottom, does not alter the case, for the gyrations, upon which the lowering of the strata of equal tension and temperature depend, are produced just the same in both cases. The stratum of the air, then, of which the tension and temperature are such as to condense the moisture of the air, assuming this shape, of course the base of the cloud assumes the same. If the dew-point of the air at the earth's surface is 10° C. below the temperature of the air, then air at the outer limit has to ascend about 1,000 meters before cloud-formation takes place, and this determines the height of the spout. The distance from the center at the base, at which condensation and cloud-formation takes place, depends upon the rapidity of the gyrations, and this upon the amount of

initial gyration and of friction. In a tall, slender column of gyrating air the friction is small, and the gyratory velocity may be assumed to be very nearly inversely as the distance from the center, except very near the center, where the gyratory velocity becomes almost infinitely great. Without any friction the waterspout would always be brought down to the earth, it might be in the form of a mere thread, however small the initial gyrations, but in nature, where friction, at least near the center, must diminish considerably the velocity of the gyrations, this is not the case. The diameter of the base of the waterspout depends upon the gyratory velocity, and where this on account of friction near the center, is not sufficient to bring the spout down to the surface of the earth, it is seen merely as a funnel-shaped cloud.

Small waterspouts which are seen upon the sea or small lakes in perfectly clear and calm weather, arise from a state of unstable equilibrium of the clear but nearly saturated air near the surface of the water. The principle of their formation is the same, but a greater rate of decrease of temperature with increase of altitude is required, than when their first formation commences up in the region of the clouds.

Cloud-bursts.—We have seen how a heavy body may be sustained and kept up in the air near the center of a tornado for a long time. In the same manner a large accumulation of rain is sustained, and prevented from being dispersed by the inflowing currents so long as the rain is not carried up where the air flows out from the center. Calculation shows that the amount of rain condensed from nearly saturated currents of air with such velocities as must exist in the central parts of tornadoes is enormous. The water cannot fall in drops on account of the strength of the current. It therefore accumulates in the body of the cloud, and especially at points where the ascending current is least, until the weight of water becomes so great that it is poured down through the air in streams. Where these streams strike the earth's surface they excavate great holes in the earth, often several yards deep, and if this occurs on a mountain side, great ravines may be produced. That these holes in the earth and ravines are caused by a stream of water, and not by a very heavy rain, is evident from the fact that the sides of these holes are often cut down almost perpendicularly, while leaves and other light substances, where these holes occur on mountain sides, remain undisturbed near the border on the upper side. The ascending current keeps rain-drops from falling, so that no water falls except in the down-pouring streams.

Cloud-bursts are most apt to occur on mountain sides. The tornado, heavily loaded with accumulated rain-water, on approaching a mountain side is very much interfered with by it.

The draught of the ascending current, as we have seen, is mostly near the earth's surface. When the base of the gyrating column of air strikes the mountain side, this draught is somewhat cut off, and the whole system somewhat broken up, and the power of the tornado destroyed. Hence the whole accumulation of water is sometimes poured down, almost at once, on the side of the mountain, tearing up rocks and trees, and causing a great ravine.

Hail-storms.—As in tornadoes, there is a stratum of air brought down to the earth by the centrifugal force of the gyrations, where the condensation of vapor into cloud and rain first takes place, and which assumes the figure of the water-spout, so very much higher up there is one brought down, it may be entirely to the earth, where the tension is so small and the temperature so low as to freeze the vapor into snow and the rain-drops into hail, even in the summer season. The altitude of this stratum, where it is not brought down to a lower level by the gyrations, depends upon the excess of the temperature of the air at the earth's surface above the freezing point. Drops of rain carried by the ascending current above this stratum, or where it is brought down to or near the earth, within it, are frozen into hail. These may be carried outward above where the ascending currents are so weak that they can fall to the earth, and as they may fall very slowly and may have been cooled down considerably below the freezing point, they may continue to increase in size all the way down by freezing the water which adheres to their sides in falling, for the ascending current would bring a great deal of rain in small drops and mist in contact with them.

Sometimes much of the hail in thus falling is drawn in toward the center by the inflowing currents from all sides below, until there is a great accumulation of hail in the center of the tornado, just as of rain in the case of a cloud-burst. If from any cause, then, the strength of these currents should become suddenly weakened, or the whole system broken up, all this hail would fall rapidly to the earth, and hence the almost incredible amounts of hail which are said to fall sometimes in a very short space of time.

A considerable amount of rain may be carried some distance up into the snow region before it has time to freeze. By the mixture of rain and snow, small balls of very moist snow are formed, which, being carried out where the strength of the ascending current permits them to fall slowly, they continue to grow until they become heavily coated with solid ice, and finally reach the earth. It is in this way that the large hailstones with a snowy kernel within are formed. But these in falling are sometimes carried by the indrawing current below

into the central part of the tornado, where the ascending currents are strong enough to carry them up again into the region of soft snow mixed with rain, where they receive another coat of soft snow, less compact than the coat of ice, after which they are thrown out again above where they fall gently down and receive another coat of solid ice. This may be repeated a number of times, the hail-stone moving in a sort of oval orbit, upward in the central part, outward above, and down at a distance from the center where the strength of the ascending current is such as to allow it to fall, and then toward the center again, to commence another similar revolution. While in the upper snow region it receives a coat of snow, and while in the region of cloud and rain, a coat of solid ice. Hence it is no unusual thing to find large hail-stones composed of a number of coatings like an onion, these coatings consisting of alternate layers of frozen soft snow and solid ice.*

Sand-spouts.—These occur mostly on dry, sandy deserts, where the surface becomes very much heated, and the rate of decrease of temperature with increase of altitude is such that the unsaturated and almost entirely dry air is in the state of unstable equilibrium. The sand-spout originates just as any small tornado, or as small waterspouts upon lakes in fair weather, but the air is so dry that there is no condensation of vapor, unless it is at a very great altitude, but the indrawing and ascending currents carry with them a great quantity of dust and other light substances, which assume the form of a pillar extending high up into the air. As occurs in all tornadoes and waterspouts, the air flows in from all sides below to supply the draught of the ascending current, mostly near the earth's surface, but also in some degree up to a considerable altitude, and these inflowing currents drive the dust which is raised on all sides, in toward the central part, and thus the dusty part of the air assumes the figure of a column.

As the particles of sand gyrate rapidly with the air, the centrifugal force of the gyrations tends to drive the particles from the center, but this is counteracted by the resistance of the indrawing currents, which is a function of the size of the particle and the strength of their currents, since it is nearly as the square of the product of the velocity of the current into the diameter of the particle. Hence, particles of sand of different sizes arrange themselves at different distances from the center, the smaller particles penetrating nearer the center, since the centrifugal force is as the cube of the diameter, while the resistence of the inflowing current is nearly as the square of the diameter. If, however, the particle were very large, it might

* See American Journal of Science, II, vol. l, p. 403.

be kept at so great a distance from the center, that the ascending current there would not be able to keep it up, so that if there were no limit to the sizes of the particles, yet there would still be a limit to the dimensions of the pillar of sand, which would be determined by the ascending velocity of the air at different distances from the center.

Water-spouts and Sand-spouts are hollow.—Near the center of the gyrations the centrifugal force is so great that the small particles of condensed vapor in waterspouts, and of fine sand particles in sand-spouts, cannot exist there, or at least they are comparatively rare, so that these spouts have the appearance of being hollow. M. Boué, in the year 1850, observed three water-spouts at the same time on Lake Janina, from the top of a high mountain. The weather was entirely clear, without clouds or wind, but very oppressive and hot. The spouts seemed to rise up from the lake, and he could look down into the top of them and see that they were hollow in the middle. (Bulletin Soc. Geologique de France, v. viii, p. 274.)

Of a whirlwind observed at Schell City, Mo., in the summer of 1879, Professor Nipher says: "There were no surface winds strong enough to bear dust along the surface of the ground, but the dust carried up in the vortex was collected only at the vortex of the whirl. The dust column was about two hundred feet high and perhaps about thirty or forty feet in diameter at the top. The direction of rotation was the same as of storms of the northern hemisphere. Leaving the road the whirl passed out on the prairie, immediately filling the air with hay, which was carried up in somewhat wider spirals, the diameter of the cone thus filled with hay being about one hundred and fifty feet at top. It was then observed also that the dust column was *hollow*. Standing nearly under it, the bottom of the dust column appeared like an annulus of dust surrounding a circular area of perfectly clear air. The area grew larger as the dust was raised higher, being about fifteen or twenty feet wide when it was last observed." (Nature, Sept. 11th, 1879.)

ART. VIII.—*Magnetic Observations made in Davis Strait, in August and September*, 1880, *on board the Steamship Gulnare;* by O. T. SHERMAN.

THE Steamship Gulnare was provided with a Lamont magnetometer, made by Fauth & Co., and a Kew dipping needle, made by Cassella. Before the starting of the expedition, both instruments were set up in the private observatory of Mr. C. A. Schott, in Washington, and the observers had the great benefit of his advice. The methods of observation, the forms of record and reduction are recorded, in part, in Appendix No. 16, Coast Survey Report, 1875, in part in the "Admiralty Manual of Scientific Inquiry." Frequently, however, it was found desirable to have recourse to the sextant to obtain the azimuth.

The first observations we record were taken at St. John, N. F., part at the private observatory of Mr. John Delaney, part on the hill forming the harbor. A local publication containing information "derived from the most authentic sources," gives the variation for 1880, as 32° 30′ West. The authority is not known. Commander Robinson, R. N., observed in 1878 a value 31° 30′. The variation chart, for 1880, published by the British Admiralty, shows the line of 31° running through the harbor. Our own value is 30° 40′. It is derived from five observations, four of which are absolutely independent. The extreme values differ among themselves by 6′·1 when reduced to the mean of 24 hours. This discrepancy I am at a loss to explain. No data are known which would refer it to local attraction. The horizontal force observed was 3·3373, the dip 74° 45′·4.

Lively, Disco Island, Greenland, formed our second station. This place had formerly been visited by Sontag in Sept., 1861, who found the dip 81° 51′ and the horizontal force 1·762, but who records no declination. It was again visited by the Alert and Discovery in 1875; the record then made the declination 67° 12′·8–68° 45′, dip 81° 56′–81° 43′·7 and horizontal force 1·770–1·805. Total force, 12·514–12·578. The remark is added that the observations showed evidence of considerable local attraction. Our record is one of disturbance only. On August 11th, the declination observed by the magnetometer varied from N. 46° 9′·7 W., at 11^h 13^m A. M., to N. 49° 15′·3 W., at 4^h 32^m P. M. On August 18th, at the same spot, but with an azimuth compass, the declination varied from N. 67° 54′·1 W., at 7 A. M., to N. 68° 52′·4 W. at 3 P. M. Our needle was consequently deflected over twenty degrees by the magnetic storm of August 11th. On several successive days also, it was

our custom, as the ship swung with the tide, to observe the errors of the ship's compass by reference to a fixed and distant mark. As yet, however, we have been unable to derive from them a series of values, which makes the ship's constants at all comparable with the same values derived elsewhere; whether from local attraction or magnetic storm, those who can refer to continued observation must determine. On August 12th, the magnetometer gave a horizontal force of 1·9042. On August 14th, in the same position as the declination of the 11th and 18th, 1·7559. On September 1st, at a station almost midway, 1·8842. These values correspond in magnitude to the distances from one of the many gneiss knobs. Feeling uncertain, therefore, as to the extent to which the observations might be affected by local attraction, more especially as observations from stations in the Waigat corresponded but poorly with those at Disco, we determined, on our return, to endeavor to discover some place which might be free from local influence. Taking the dipping needle, we made observations from the top of the hills to the sea coast. Placing these on the chart they are found to increase in value on either side of a knoll of trap rich in magnetite, on which the dip was 80° 48′. Half way up the hill it became 81° 6′, on the top of the hill, 81° 23′. Speaking generally, the line 81° 50′ runs from a point half way between Wildfire and Englishman's Bays, along the inner shore of the island forming the harbor. The line of 82° runs through the middle of the western part of the island and on the sea shore on the eastern. The line of 82° 6′ on the western sea shore. All lines form a loop in the direction of Crown Prince islands. We found no spot free from local influence. A stone was brought to me while here, which both Prof. Steenstrup and myself recognized at once as "Ovifak meteoric iron." It was said to have been found in Wildfire bay. From what we now know, however, it seems more likely to have been brought by the natives from Ovifak. They keep a number of these stones on hand for purposes of trade.

At Rittenbenk, lat. 69° 44′, long. 51° 2′ W., we found on August 23d, 1880, the dip to be 81° 53′·9, the total force 12·6213, and the variation N. 70° 2′·9 W., at 11^h 30^m local time. The Alert gives for this station a declination of 69° 8′·5 at 6·40 P. M. The station is granitic and there may be local attraction.

At Sakkak, lat. 70° 1′ N., long. 51° 55′ W., we found on August 24th, the dip to be 81° 59′·6, variation N. 70° 47′·3, at 12^h 15^m local time; and on August 31st, the horizontal force 1·7904. This station is also probably affected by local attraction.

At Kidluset, lat. 70° 10′, long. 53° 0′, August 25th, 1880, we observed a dip 82° 11′·8, and total force 12·5435. These are probably not affected by local influence.

The Gulnare was a wooden ship with iron frame. She had seen many years' service in the waters of New Foundland, but during the winter before the expedition sailed, had been almost entirely rebuilt. She was swung at Hampton Roads, on June 23d, 1880. The observations discussed by the method of least squares give the value of the ship's force to head, −1·8403, to starboard, −0·7845. Three days after, the salt which the engineer had allowed to collect in the boiler reached a thickness of several inches and the fire boxes collapsed. These were replaced at St. Johns and for ten days and nights the iron in that part of the ship was again subjected to hammering. The ship was again swung at St. Johns. The value of the ship's, force reduced, after Evans, by the least squares are force to head, −1·916, to starboard, −0·2599, to nadir, −0·4081. On August 30th, the values were, force to head, −1·46299, to starboard, −0·83918. On October 5th, the values became to head, −0·9971, to starboard, −1·4525, to nadir, −0·3907. A change I should be loath to accept were it not thrust upon me by the facts of navigation. The swing of October 5th was necessitated by the discrepancy between the observed and calculated courses. It was our custom at sunrise or sunset to observe the angle between the sun's limb and the line of the ship's keel, noting at the same time the ship's heel and course by the disturbed compass. These observations served at the time to correct our course. Several of these have been again employed to give us the declinations at the place of observation. The ship's forces for the date were obtained by simple interpolation from the values above given. These connected with the soft iron coefficients give us readily the values of the semi-circular variation. These, finally, we have placed in the exact expression

$$\sin\delta = \frac{AC \pm B\sqrt{-C^2+A^2+B^2}}{A^2+B^2}$$

which is readily deduced from Evans' well known formula. A, B and C are here easily calculated functions of the semi-circular and quadrantal coefficients, and the ship's apparent azimuth. The sign + being taken, when the compass reading is from N. 0° E., to N. 180° E., the sign − for the remaining readings. The values obtained in this way are as follows:

Date.	Lat. N.	Long. W.	Hour, P. M.	Declination.
August 5, 1880,	62° 30′	51° 45′	8 23	N. 57° 42′ W.
September 10,	67° 6′	58° 30′	6 43	N. 70° 59′ W.
September 14,	59° 30′	56° 26′	6 27	N. 57° 29′ W.

ART. IX.—*On the Crystalline form of Sipylite*; by J. W. MALLET.

IN the original description* of the mineral in question from the allanite locality in Amherst Co., Va., very little could be said about the crystalline form, as but a few imperfect faces had been met with. I have recently obtained some additional specimens, most of them irregularly shaped nodules imbedded in allanite, but fortunately among them one nearly complete detached crystal, broken into two parts indeed, but these fitting accurately together, so that the form can be easily made out.

This little specimen is a tetragonal octahedron, 1·5 centimeter long, weighing 1·627 grm. No faces are visible save those of the octahedron (1) and faint indications at one or two places of an extemely narrow plane replacing its terminal edges. The surfaces are too dull to allow a reflecting goniometer to be used, but an application goniometer gives the angles

$1 \wedge 1$ (over summit)$=53° 0'$
(Hence $O \wedge 1=116° 30'$)
$1 \wedge 1$ (adjacent pyramidal)$=100° 45'$
$1 \wedge 1$ (basal)$=127° 0'$

These measurements show a close relation to fergusonite, for which

$O \wedge 1=115° 46'$
$1 \wedge 1$ (pyramidal)$=100° 54'$
$1 \wedge 1$ (basal)$=128° 28'$

The relation in form between fergusonite and tapiolite and xenotime on the one hand, and scheelite, stolzite and wulfenite on the other has been pointed out by Rammelsberg.† The angles for sipylite and for fergusonite are connected with those of xenotime if a of the two former be taken $=2a$ of the latter, and this,‡ as well as Rammelsberg's analysis of fergusonite, supports the view expressed in my former paper that sipylite is an ortho-niobate—$R''_3 M^v_2 O_8$—containing basic hydrogen.

The sipylite crystal shows distinct cleavage parallel to 1. It is fully identified with the mineral originally examined by its general physical characters. The sp. gr. $=4·883$ at 16° C.; formerly found, 4·887 at 12°·5, and 4·892 at 17°·5.

Univ. of Virginia, May 21, 1881.

* This Journal, 397, Nov., 1877. † Jour. Chem. Soc., 189, March, 1872.

‡ Taking the usual, and probably correct, view of xenotime, that it is an orthophosphate. But no great weight can be attached to any opinion as to yttrium compounds until the confusion at present existing in relation to the metals which have together passed under this name has been cleared up.

ART. X.—*Observations on the Structure of Dictyophyton and its affinities with certain Sponges;* by R. P. WHITFIELD.

IN the Chemung group of New York, and in the Waverly beds of Ohio and elsewhere, there occurs a group of fossil bodies which have been described under the name *Dictyophyton*, but the nature of which I think has not been properly understood. In the 16th Report on the State Cabinet of Natural History of New York, page 84, in the remarks preceding the generic description, they are referred to the vegetable kingdom with the opinion expressed, "that they are Algæ of a peculiar form and mode of growth." A reference which I think their nature does not warrant.

If one examine the figures of the various species described, given on Plates 3 to 5A of the above cited work, it will be seen that these bodies are more or less elongated tubes, straight or curved, cylindrical or angular, nodose or annulated; and that they have been composed of a thin film or pellicle of net-work, made up of longitudinal and horizontal threads which cross each other at right angles, thereby cutting the surface of the fossil into rectangular spaces; often with finer threads between the coarser ones. When the specimens, which are casts or impressions in sandstone, are carefully examined, it is found that these threads are not interwoven with each other like basket work, or like the fibers of cloth, nor do they unite with each other as do vegetable substances; but one set appears to pass on the outside, and the other on the inside of the body. The threads composing the net-work vary in strength, and are in regular sets in both directions, while the entire thickness of the film or substance of the body has been very inconsiderable. In one species, the only one in which the substance filling the space between the cast and the matrix has been observed, it appears to be not more than a twentieth of an inch in thickness, and is ochreous in character. This peculiar net-like structure does not seem to be that of any known plant, nor does their nodose, annulated, cylindrical or often sharply longitudinally angular form, with nearly perfect corners, indicate a vegetable structure; moreover, it is not a feature likely to be retained in a soft, yielding vegetable body of such extreme delicacy and large size, while drifting about by the action of water, in becoming imbedded in the sand of a sea bottom, but would rather indicate a substance of considerable rigidity and firmness of texture.

In examining the structure of *Euplectella* it is found to be composed of longitudinal and horizontal bands similar to those above described, with the additional feature of sets of fibers

passing in each direction obliquely across or between the longitudinal and horizontal sets, but not interwoven with them; so that the longitudinal series forms external ribs extending the length of the sponge, and the horizontal series inside ribs or bands, and they appear as if cemented to each other at their crossings. The oblique threads, besides strengthening the structure, cut across the angles of the quadrangular meshes formed by the two principal sets of fibers, and give to them the appearance of circular openings, making the structure much more complicated than in *Dictyophyton.* The addition of oblique fibers in *Euplectella* is the most noticeable difference between the two forms; but if placed horizontally and longitudinally between the primary sets they would produce precisely the structure seen in *Dictyophyton.*

As yet we have no positive evidence of the nature of the substance which composed the fibers in *Dictyophyton.* The only cases known, so far as I am aware, of the preservation of the substance of the fossil is that mentioned above, where the space between the matrix and the cast is occupied by a ferruginous body, a material which so often replaces siliceous organisms in a fossil state, and specimens of *D. Newberryi* from Richfield, Ohio, on which there occur slight patches of a carbonaceous substance, but not sufficient to warrant the conclusion that it ever formed a part of the structure, even in the opinion of the author of the genus who supposed these organisms to have been of vegetable origin; especially as they are associated with numerous fragments of terrestrial plants. I am therefore led to the opinion, from their firmness of texture as evinced by the strong markings left in the rock, and the almost perfect retention of their original form, that they were of a siliceous nature. Still, in this opinion I may be mistaken, and it must be left for future discovery to determine; but that they were of the nature of sponges and not of plants I feel very confident.

The form given by Professor Vanuxem in the Geological Report of the Third District of the New York Survey, and also figured in the 16th Report above cited, I think would also better conform to this idea than to that of a vegetable origin, although its broad flattened bands may be something of an objection.

The name *Hydnoceras* was originally applied by T. A. Conrad to designate a species of this genus (Jour. Acad. Nat. Sci. Philad., vol. viii, 1st series, p. 267), but was discarded on account of its objectionable signification, though if the view here suggested prove correct the later appellation is almost as objectionable.

ART. XI.—*The Carboniferous Rocks of Southeast Kansas;* by G. C. BROADHEAD.

AT the eastern boundary of Miami County, Kansas, we find the high lands to vary from 950 to 1050 feet above the sea, the valleys being 875 to 910. In the Neosho Valley the elevation at Neosho Falls is about 1000 feet. Up to this place and a little farther we pass over a gently sloping country. It then rises more rapidly, being 1150 feet on higher land. West of the Verdigris the country rises more rapidly and is more rugged.

In Osage County coal is profitably mined, which, according to Prof. Mudge belongs to the Lower Coal-measures. The Lower Coal-measures pass southwardly along the Neosho Valley which seems to occupy a trough in these measures, but eastwardly, including Miami County, the northern half of Anderson and the county northwardly, only the upper series are exposed, connecting with similar measures in Missouri.

West of the Verdigris River the Upper Coal-measures also extend but soon disappear beneath the "Permian." The main productive Coal-measures of Southeast Kansas lie south of Miami County. Passing from Paola southwestwardly to Greenwood County, we find only a thin coal-seam occasionally mined but with no profitable result. Near the line of Greenwood and Woodson Counties a seam of less than a foot thickness is sometimes mined. This is the most western exposure of coal belonging to the Carboniferous formation. In the western part of Woodson and in Greenwood County the lowest exposed rock is 50 feet of coarse sandstone which I have referred to the Lower Coal-measures, but only a few fragmentary remains of plants were found in it. Above this are thin limestone beds full of *Fusulina cylindrica* and nearly 200 feet more of sandstone, with other limestone beds above, containing well known Carboniferous fossils, including *Fusulina cylindrica* and *Chœtetes*. The step now is more rapid to the "Permian."

Entering the State near the line of Cowley and Chautauqua counties, we find ourselves upon a long dividing ridge extending and well defined for seventy miles northwardly.

This ridge is much higher than the country either east or west of it, and is known in southern Kansas as the "Flint Hills," on account of numerous fragments of flint lying strewn over the surface. It includes the Permian rocks of Kansas and might appropriately be termed the "Permian Mountains." Its elevation above the sea is 1560 feet near Greenfield, in northeast part of Cowley County 1600 feet; and the highest point near the corner of Greenwood, Elk and Butler about 1700 feet. This is the

highest ground east of Arkansas and Walnut Valley. On the west side of this ridge the descent is gentle and scarcely perceptible, being 390 feet in 25 miles to the Arkansas Valley. On the east the descent is more abrupt, the ridge presenting rugged walls of limestone separated by shaly slopes, and the hills descend 350 feet in four miles or 390 feet in six miles, and in some places the descent is still more abrupt. From the main ridge sharp spurs extend off from six to ten miles eastwardly. From the peculiar rough character of the eastern face of this ridge good wagon passes are often distant as much as ten miles.

The approaches to this ridge from Fall River Valley is by a succession of terraces or plateaus of upper Carboniferous rocks. At Twin Falls we are on a lower terrace elevated about 1000 feet above the sea. The second terrace is reached six miles southwestwardly at 1160 to 1180 feet. This terrace occupies a large area of the eastern part of Greenwood County with most of Elk. The elevation of the next terrace is about 1300 feet above the sea and it reaches to the foot hills of the Permian and the slopes above blend with the Permian. This will include altogether about 500 feet of Upper Coal-measure rocks in this part of Kansas which lie below the Permo-carboniferous. These beds are mainly shaly sandstones with occasional limestone beds, and as far as observed contain one coal bed of seven inches with two beds of bituminous shale, and one other coal seam of five inches thickness appears just beneath the Permian. The Permian or Permo-carboniferous of the "Flint Hills" include a total of about 500 feet thickness. The following section I have condensed from several taken within twenty miles.

1. Sixty-two feet including chert layers with thin beds of shaly drab-colored limestone: the highest rocks seen in "Flint ridges," observed *Bryozoa* with *Athyris subtilita*, *Productus costatus* and *Hemipronites crenistria*.

2. Ninety feet mostly thin limestone layers chiefly disintegrating on exposure.

3. Seven feet bed of porous chert resting on limestone. *Pinna peracuta* found everywhere. A *Phillipsia* was also obtained.

4. Eighty-five feet chiefly drab shales with some thin layers of limestone and red shale near lower part. Fossils are very abundant and can be picked up in a finely preserved state, and include *Fistulipora* (?), *Productus Nebrascensis*, *P. semireticulatus*, *Meekella striaticostata*, *Chonetes graculifera*, *Terebratula bovidens*, *Athyris subtilita*, *Yoldia subscitula*, *Schizodus Rossicus*, *Myalina perattenuata*, *Hemipronites crenistria*, *Aviculopima Americana*, and other known Upper Carboniferous fossils.

5. Five feet of bluish drab and sometimes buff limestone containing *Eumicrotis Hawni*, *Myalina perattenuata*, *Aviculopecten occidentalis*. [This bed is easily recognized wherever seen.]

6. Ten feet red and green shales.
7. Fifty-three feet beds shale, with some beds of limestone very good for building purposes.
8. Twenty-eight feet limestone abounding in *Fusulina cylindrica;* the middle layers contain blue chert full of *Fusulinæ* showing the structure very finely.
9. Twenty-eight feet of sandstone.
10. Four feet gray limestone containing *Productus semireticulatus*, *Allorisma granosa*, *A. subcuneata*, *Pinna peracuta*, *Nautilus capax*, etc.

The last bed I regard as the base of the Permian.

Other fossils obtained at the several localities include *Allorisma subelegans*, *A. Topekaensis*, *Macrodon* —, *Nautilus occidentalis*, *Murchisonia* —. Although these fossils seem at home in the *Permian*, I have obtained them also, with scarcely an exception, from known Upper Coal-measure rocks of Missouri; in fact most of them have been obtained from the rocks of Kansas City.

The limestones of the Permian have been extensively quarried in Kansas from the southern to the northern part of the State, and many tons sent off to the market. Some of the rock quarried is too soft for valuable structures, but many very excellent quarries have been opened.

From levels taken on corresponding beds wide apart, we find there is a regular dip westwardly of not less than 25 feet per mile. Assuming this to be correct we may be safe in saying that there are 1500 feet total thickness of Permian beds in southern Kansas. In the counties of Butler, Cowley, Elk and Greenwood, it is the *newest* rock below the *Quaternary*. No other rocks of later formation than the Permian are found here. The PERMIAN of Kansas rests *conformably* on the *Coal-measures* and there is no decided line of separation between the two. Certain strata can be grouped together as can certain other strata of other formations.

The only marked difference is this: Passing a certain horizon in the ascending series, we find the rocks to be all of a drab, buff or cream color and the limestones more impure and breaking with a rough fracture, and when vertically jointed the angle more nearly approaches a right angle, whereas the Coal-measure limestones are generally more acutely jointed and the blocks are regular rhomboids.

The group of the PERMIAN MOUNTAINS forms an interesting study; the strata are easily traced and the scenery afforded is very fine and views extensive.

The above is an abstract of a more detailed paper.

ART. XII.—*The Later Tertiary of the Gulf of Mexico;* by E. W. HILGARD, Berkeley, Cal. With a map (Plate III).

IN view of the late publication of the Coast Survey chart of soundings in the Gulf of Mexico, and of the observations of Dr. Eugene A. Smith on the Geological Formations of Florida (this Journal, April, 1881), I desire to summarize briefly the facts upon which my hypothesis of a temporary and partial isolation of the Gulf from the Atlantic Ocean during the later portion of the Tertiary period, is based. I shall add thereto some additional facts that have since been brought to my knowledge, concerning the more remote portions of the group of deposits to which, from its most accessible and representative exposure at the town of Grand Gulf, on the Mississippi River, I have given the name of "Grand Gulf Group."

So far as known at present, the "Vicksburg" group of marine marls and limestones, containing only extinct forms of life and therefore according to usage accounted "Eocene," closes abruptly the Tertiary series of marine fossiliferous deposits, on the entire mainland border of the Gulf of Mexico, from Florida to the Rio Grande. In the portions lying near the main axis of the Mississippi trough, the uppermost strata of the Vicksburg rocks show, by the constant intercalation of laminated clays and lignite beds and seams with the marine deposits, that the sea was shallowing more and more; and the highest portions are everywhere in the State of Mississippi characterized by a great prevalence of gypsum seams, and are often strongly impregnated with magnesian salts, as well as with common and Glauber's salts. The same is true of the lower portions especially, of the overlying Grand Gulf rocks; so that throughout the region occupied by the latter, few well-waters obtained within them are fit for daily use, and many are strongly mineral.

At their lines of contact, the Vicksburg and Grand Gulf rocks consist almost throughout of lignito-gypseous, laminated clays, passing upward into more sandy materials: they are not sensibly unconformable in place: but while the Vicksburg rocks show at all long exposures a distinct southward dip of some three to five degrees, the position of the Grand Gulf strata can rarely be shown to be otherwise than nearly or quite horizontal on the average; although in many cases faults or subsidences have caused them to dip, sometimes quite steeply, in almost any direction. They, however, lie high on the hill-tops between the towns of Vicksburg and Grand Gulf, and disappear at the water's edge near the Louisiana line, under the gravel beds of the Stratified Drift.

The latter is found directly capping, almost everywhere, the claystones and sandstones that characterize the highest part of the Grand Gulf group. Clearly, the Grand Gulf rocks alone represent, on the northern border of the Gulf, the entire time and space intervening between the Vicksburg epoch of the Eocene, and the Stratified Drift. Their total thickness does not exceed, if indeed it reaches, 250 feet. In the absence of deep borings on the Grand Gulf territory, this can be best observed on the northern edge of the formation, where it forms high ridges, from which there is an abrupt descent, northward, into the level prairie country of the Vicksburg territory.

From these rocky hills, which form sharp ridges diagonally across the States of Mississippi and Louisiana, and a portion of Texas, and which present even in small profiles an indefinite variety of more or less laminated claystones, clay-sandstones, or sometimes siliceous sandstones, there is a gradual descent southward, and a gradual increase of clayeyness and decrease of hardness, until, in the seaward portions of the formation, we find chiefly stiff, blue or green, and more or less massy clays. In these, at a certain level, there occurs a stratum copiously traversed by calcareous seams; and smaller ones occur at higher levels. In one such outcrop, on Pearl River, I found the only vestige of a zoögene fossil thus far seen in the entire formation; it is recognized by Professor Marsh as a fragment of a turtle shell. Apart from this, my most patient search, in hundreds of localities, has failed to produce any definite fossil form; even the leaves associated with the lignite seams being so ill preserved as to be unrecognizable.

While in Mississippi and Louisiana the calcareous facies is altogether exceptional and local, a few square miles of black prairie (Anacoco Prairie) in western Louisiana being its only striking manifestation east of the Sabine, it seems to become almost predominant in middle and southern Texas. The black calcareous prairies of that portion of Texas lie in bands sensibly parallel to the coast, each band differing somewhat in character from the rest, on account of its soils being more or less directly derived from the materials of the underlying formations. These are successively, counting from the coast landward: the Port Hudson (Champlain), Grand Gulf, Vicksburg, Jackson (Tertiary), and finally the Upper Cretaceous beds. This state of facts, my knowledge of which was until lately based only on scattered data gathered here and there, has received detailed confirmation from the observations made by Dr. R. H. Loughridge in 1879, on a reconnoissance of the State made in connection with the agricultural investigations of the Census.

It is thus placed beyond doubt that the Grand Gulf rocks form a continuous belt, from the Perdido River on the western

line of Florida (where according to Dr. Smith the Vicksburg rocks reach the coast) to the Rio Grande; attaining a width of a little over a hundred miles in the axis of the Mississippi trough, southward of Vicksburg, and thence narrowing rapidly to an average width of forty miles in Texas, and crossing the Rio Grande with an approximate width of 150 miles. What becomes of it beyond the latter line, is a matter of conjecture.

Of the sweep of about 900 miles thus outlined as the known extent of this formation, about 400 may be considered as having been examined sufficiently in detail to prove the absence of marine fossils from the formation; the portion so examined embracing, moreover, its widest part and fully two-thirds of the area of outcrop.

I have heretofore (this Journal, Dec., 1871) remarked that such absolute dearth of fossils in a formation whose materials are so well adapted to their preservation, staggers belief; and that I interpret the calcareous seams and concretions, found in some portions of the formation, as derived from the long-continued maceration of an apparently copious fauna; as is exemplified in the Quaternary beds of Côte Blanche on the Louisiana coast, and notoriously in the limestones of the coral reefs.

But even upon this basis two points confront us in the discussion of the relations of the formation to the sea: the great rarity of the calcareous feature in the main body of the formation; and the utterly "unmarine" character of the materials generally, in the constant recurrence of the lignito-gypseous facies.

The first objection disappears, as just stated, in the south Texan portion of the area. Curiously enough, precisely the same thing happens in the case of the Quaternary strata of the Texan coast, whose direct connection with the "Port Hudson" strata of Mississippi and Louisiana is indisputable. Specimens collected by Dr. Loughridge on the coast at Port Lavaca, and according to him fairly representative of the general facies of the shore in that region, show that the subordinate feature of the fresh-water limestone ledges seen on the Louisiana coast, has here become quite prevalent. But here, also, fossils are very scarce at least, for he was unable to find a single recognizable form at any of the outcrops examined by him. It would thus seem as though we were driven to account for the same state of things in the Quaternary as well as in the later Tertiary period—the absence of marine deposits and fossils, where on ordinary grounds of probability we should expect to find them; and their replacement by fresh- or brackish-water deposits, with fossils macerated to unrecognizability.

To complement this statement of facts, while unable to find

any definite data to show the geological features of the region beyond the Rio Grande, I call attention to the fact that the edge of the Mexican plateau approaches the coast most closely to landward of Vera Cruz. At that place, the castle of San Juan De Ulloa stands on a rock which, from specimens brought home by soldiers from the Mexican war, I then understood to be a freshwater limestone, full of helices, or shells resembling them. If there be any more definite data extant on this point, I should be glad to have them pointed out. It seems almost incredible that so obvious a feature of a seaport so frequently visited by Americans should not have been better observed, even accidentally.

The geology of Yucatan is involved in equal obscurity. The casual statements made as to the nature of the rocks by travelers, are too indefinite to afford any clue upon which conclusions might safely be based.

As to Cuba and the rest of the Antilles, we do know that their shores are lined with marine fossiliferous Tertiaries, much disturbed by the upheavals that have occurred. We even have descriptions, and quite a long list of names, of fossils found in these formations. But on the one hand, the English observers have taken the futile pains of comparing these beds with European Tertiaries only; while Mr. Gabb, true to the time-honored idea of making as many distinct species as possible, has in his descriptions of the Tertiaries of Santo Domingo given us the impression of the creation of a new fauna specially for that island, with scarcely an attempt to identify the variations of forms there found, with those already known from the other Tertiaries of the Gulf border. Moreover, the tendency of most observers to pass lightly over the unconformable, difficult deposits of the Quaternary, in which no glory can be gained by describing and naming new species, has left us with but a faint idea even as to the presence or absence of such beds on the Antilles. I shall therefore not attempt the unpromising task of a discussion and comparison of what is known of their geology, with the known facts on the mainland of the United States.

How are the latter to be reconciled with the now well-ascertained great depth of the Yucatan Channel, and the at least not inconsiderable depth of the Straits of Florida? It seems scarcely possible to assume that both of these have been formed *de novo* at the end of the Tertiary period; nor even that the depth of the Yucatan Channel could have been so materially less since the Eocene time, as to allow of the freshening of "Sigsbee Deep" by the influx, whether of the regular drainage of the Continent, or of the contents of the receding great lakes of the plains. But the matter assumes

a different aspect when viewed by the light now afforded by our knowledge of the configuration of the bottom of the Gulf, and of the oscillations of level to which at least its northern shore, and especially the central portion of the Mississippi Valley, have been subject in Tertiary and Quaternary times.

I cannot but express my regret that the latter portion of these data should thus far rest almost alone upon my personal observations and conclusions. It seems to me that as the only ocean basin not separated from the central part of the North American Continent by areas of disturbance and mountain-making, the Gulf of Mexico deserves first and chief attention, as the reference plane from which the oscillations of that central portion must be measured: while its shores are the nilometers upon which those movements can alone be found recorded. It would seem as though the reading and exact understanding of that record should have been the first thing to be done in attempting to unravel the Tertiary and Quaternary history of the country lying between the Alleghenies and the Rocky Mountains: just as the measurement of a base line is the first in a geodetic survey. The stratified drift of the South alone renders intelligible the succession of events that must have occurred at the North: it is only on the shores of the Gulf that the question whether the Glacial epoch of the interior was one of elevation or of depression, together with the measure of these, can be finally determined. I have vainly sought for assistance in this wide and important field, until quite lately, when the explorations of Smith and Loughridge, under the auspices of the United States Census, have furnished important additional data.

The state of the evidence regarding these oscillations may be thus summarized: A comparatively rapid upward movement of the bottom of the Mississippi trough during early Tertiary time, is conclusively shown by the rapid decrease of the depth of the Mississippi embayment, which from its head near Cairo to about the mouth of the Arkansas, is filled with lignitiferous clays with only here and there a small marine estuarian deposit: except that in the State of Arkansas, a residuary basin of the old (Cretaceous) trough retained deep-sea features until the beginning of the "Jackson" epoch. The latter, with its abundant marine fauna, headed by the great Zeuglodon, was evidently deposited on a comparatively steep slope forming the southern edge of the plateau that existed in the upper portion of the embayment: yet it also consists, in the main, of clayey materials largely intermixed with lignito-gypseous beds. The succeeding "Vicksburg" stage is more of a deep-sea character, and its inconsiderable thickness in Mississippi and Louisiana speaks of a short duration of the epoch, at the end of which the lignito-gypseous feature again appears.

About that time, as E. A. Smith's late observations show, the Peninsula of Florida emerged from the water, apparently in the prolongation of the upheaval which traverses the State of Georgia from Atlanta to its southeast corner, forming the great "divide" between the rivers flowing directly to the Atlantic, and those tributary to the Gulf. This axis of upheaval, I am informed by Dr. Loughridge, is marked by numerous and very long trap dykes, running parallel to it in the metamorphic region of the State. As Dr. Smith has observed, there is a distinct ridge or "back-bone" of Florida, formed of the Orbitoides limestone, that does not lose itself entirely until the Everglades are reached. On the Florida shore, the Vicksburg rock is mostly covered to a greater or less depth by the Quaternary coralline rock, though outcropping at Tampa and a few other points.

Subsequent to this upheaval, the Miocene and Pliocene beds were deposited on the Atlantic side of the peninsula, as they were on the rest of the Atlantic coast. Meanwhile, what happened on the Gulf side?

As we have seen, the Grand Gulf beds were being deposited during that time, or a part thereof, in the axis of the Mississippi trough, and all around the Texas shore to the Rio Grande, and doubtless beyond. Toward the east, these beds "run out" on or about the Perdido River, on the line between Alabama and Florida.

A glance at the map of the Gulf soundings will show that this places the western line of the outcrop of the Vicksburg rocks exactly in the prolongation of the edge of the great submarine border plateau outlined by the "100-fathom line," from which there is such a sudden descent, all around the Gulf, into deep water.

It may be premature to infer from this coincidence, that if the Gulf shores should be elevated to the extent of 600 feet all around, we should find it lined with a wall of "Vicksburg" limestones. But however that may be, the existence of this great shelf furnishes, as it seems to me, an explanation of the "Grand Gulf" rocks on the mainland.

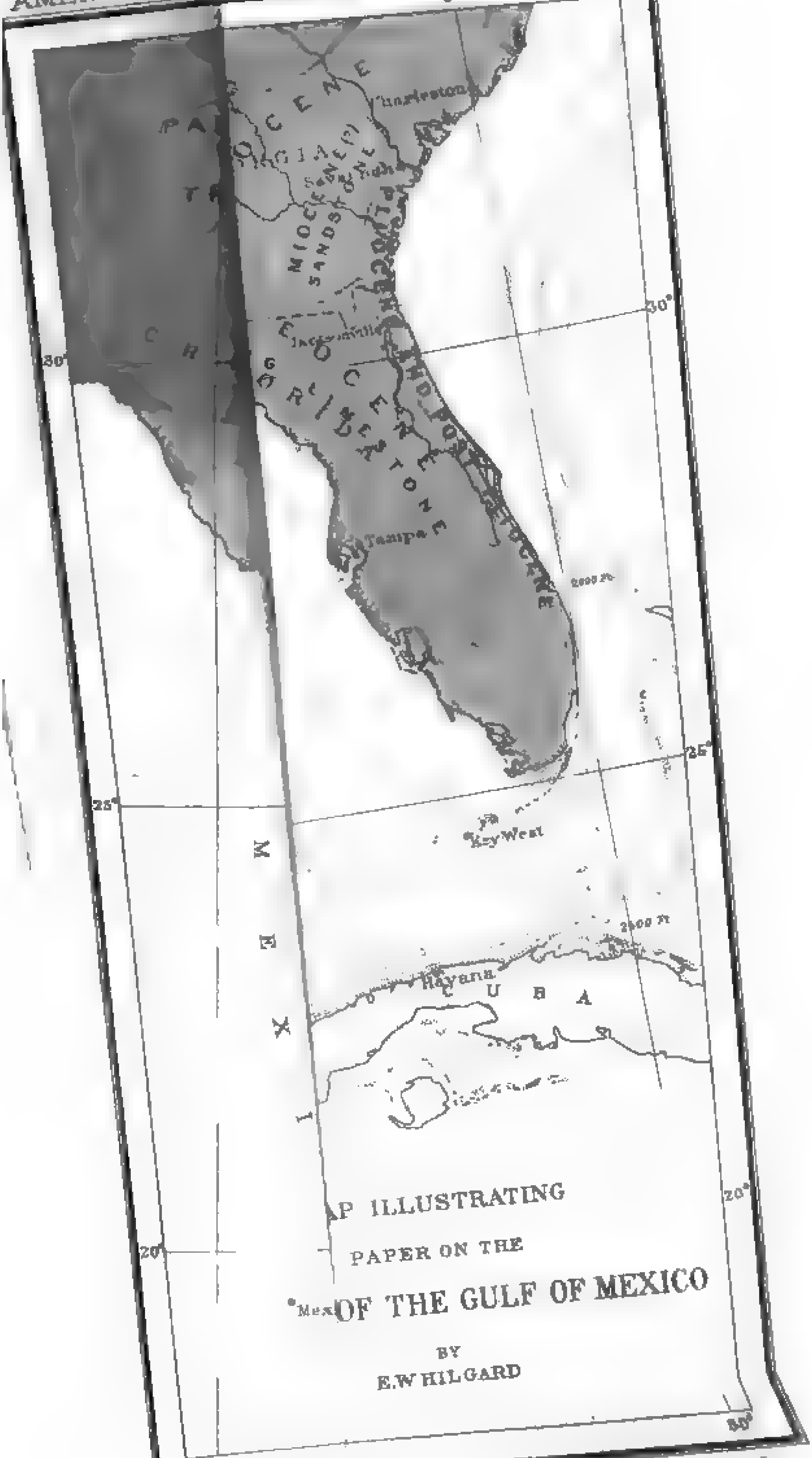
I take it for granted that the oscillations in the axis of the Mississippi Valley are proven to have been greater than on either side of the same; in other words, that it is, and has been, an axis of weakness and disturbance. As to the extent of its vertical movements in later Tertiary and Quaternary times, I have elsewhere shown that it cannot have been less than 900 feet between the time at which the great drift floods carried the northern pebbles to the Gulf shore, and that at which the lœss of the Mississippi Valley was deposited. For we find the drift pebbles at a depth of 450 feet *below* the

waters of the Gulf, in the deep wells of Calcasieu; and the lœss lies at a similar height *above* the sea-level, not many miles above the head of the Mississippi Delta.

The inference is irresistible, that the upward movement of the Tertiary period continued up to the end of the Glacial epoch, whose gravel could not be carried far beyond the shores of the Gulf. It is clear, also, that even a minimum elevation of 450 feet, so far proven, would convert the Gulf border, to the edge of the 100-fathom line, into a region of shallows, whose waters would be kept perceptibly freshened by the continental drainage, especially in the axis of the Mississippi Valley, even in the present condition of the straits of Yucatan and Florida. If, however, we suppose the bottom of the latter to have participated in the elevation to a greater or less extent, sensibly lessening the oceanic circulation, the freshening of the border waters may readily be supposed to have been such as to render very precarious the existence of either a marine or fresh-water fauna; thus accounting for the remarkable dearth of fossil forms in the Grand Gulf strata. An occasional cessation of the movement, or other local cause, might for a time allow of the existence of limited areas of abundant life, such as are indicated by the subordinate calcareous basins with, presumably, a macerated fauna. That these indications should increase as we approach the Yucatan channel, that is, along the ancient coast of Texas, is to be expected; and it may be fairly presumed, that, farther to the south, near Vera Cruz and beyond, we shall hereafter find the purely marine equivalents of the Grand Gulf rocks. That these rocks should have an exceptional character, that of coarse sandstones, near the axis of oscillation, is intelligible enough. It appears, however, that the sandstone character, which in Mississippi disappears about half way across the State, continues in Texas as far south as Indianola, and probably even to the Rio Grande, where, as previously mentioned, the formation seems to widen out even more than is the case in Mississippi. It would thus appear that Texas has participated, far more than Alabama, in the oscillations of the Mississippi Valley.

It should not be forgotten that in the latter, we find the Grand Gulf rocks, still capped by drift beds, at elevations of at least 500 feet above the Gulf. During the highest elevation of the Glacial epoch, therefore, they must have risen to over 900 feet above the sea, and in the reverse movement, of the Champlain epoch, they were again covered by the lœss and surface loams, to be re-elevated during the "Terrace" period of erosion, by which the present channel of the Mississippi River was formed.

The map of soundings exhibits very strikingly the analogy



·ius Bien, Lith.

of the relation of the two peninsulas of Florida and Yucatan to the Gulf Stream on the one hand, and the basin of the Gulf on the other. The eastern shores of both fall off steeply into deep water, while the gulfward shores are bordered by the shelf, 100 to 130 miles in width, which breaks off into deep water at the 100-fathom line. It would thus seem a priori probable, that both peninsulas were elevated at the same time and to a somewhat similar extent as regards their lowlands; and if so, this event cannot but have exerted a considerable influence in diminishing the volume of the Gulf Stream passing inside of Cuba, and in greatly restraining the peripheric Gulf current. Such events could not have failed to exert some influence upon the climate of the regions concerned, as well as upon the nature of the Gulf-border deposits.

Cannot something be done toward a prompt solution of this interesting problem in American Geology, upon which depend so many other mooted questions of first importance? A single season's yachting excursion along the shores of Mexico would, under the hands of a well-posted observer, be amply sufficient to settle all the main points. Even a few specimens of rock from prominent points might go far toward the elucidation. But any such exploration should be made, not with a view to the discovery and naming of new fossils, but with that of working from the base-line of the well-observed facts and regions toward those yet to be observed, and of unifying that which of necessity must have been evolved as a unit. That in order to accomplish this end, the weary catalogue of spurious species that now encumber our lists of Tertiary shells, must be thoroughly revised from the present biological point of view, is unfortunately true. Nowhere would a richer field reward the labors of the faithful worker. The *time* for this has certainly come—but where is the *man?*

ART. XIII.—*On Dufrenite from Rockbridge County, Va.*; by J. L. CAMPBELL.

DURING the summer of 1875, a number of specimens of iron ores from the Blue Ridge range in Rockbridge County, Va., were brought to my office for examination. One of these at once arrested my attention by its peculiar structure, color and luster. It had been taken from the mine in which it occurs partly in the form of irregular nodules, and partly as incrustations on the surface of an underlying bed of limonite. When broken open, the newly exposed surface showed a radiated, coarsely fibrous structure, with a rather dull silky luster, and a dark greenish brown (almost black) color. Where the surfaces

of the incrustations and nodules had been long exposed to the weather, the fibrous crystals had become changed in color to a yellowish brown, so as to resemble in general appearance fibrous limonite—the original structure being preserved.

The unaltered part of the mineral reduced to fine powder was of a light yellowish green color. When heated in a closed tube, it gave off water freely; and small fragments, heated to redness for a short time, assumed a bright reddish chestnut-brown color when cold. Before the blowpipe, it fused readily to black magnetic beads. With the borax bead the reactions of iron were well marked, with some indications of manganese. The mineral dissolved readily in hot hydrochloric acid. Tests applied to the solution indicated the presence of ferric oxide in abundance, and ferrous oxide in smaller quantities; while reactions of phosphoric acid were very decided.

A subsequent analysis of a choice specimen gave the following results: Specific gravity, 3·382; hardness, about 4:

Phosphoric acid (as pentoxide)	31·761
Ferrous oxide	6·144
Ferric oxide	50·845
Alumina	0·212
Manganous oxide	0·403
Lime	1·124
Magnesia	0·762
Water lost at red heat	8·531
Insoluble silica—very fine sand	0·115
	99·897

Some samples more recently tested left but a trace of silica when dissolved in hydrochloric acid, while others gave less lime and magnesia, and more alumina than the foregoing analysis indicates. Still, there is no reason to doubt that the great body of the mineral mass is "dufrenite," which hitherto seems rarely to have given identical results in the hands of any two analysts.

Geological position.—On visiting the locality where the dufrenite is found, it was ascertained to be about ten (10) miles east of Lexington, Va., near the crest of what is locally known as "South Mountain"—one of the many primordial broken ridges that skirt the northwestern base of the main Blue Ridge. It is in the ferriferous bed of shales and shaly sandstones that here constitutes the upper member of the primordial or Potsdam group. Its position will be readily understood by reference to a profile section of the Blue Ridge and Great Valley, published in this Journal for July, 1879, vol. xviii, page 19. That section cuts the range only a few miles to the southeast of Irish Creek, while the bed of dufrenite is a little to the northeast of the same stream. But if the stratum on the section marked 1*g* be conceived to extend nearly to the top of

that marked 1*f*, its upper limit would very well indicate the geological locality of the mineral deposit. The strata here, however, have a much more moderate dip than at the point cut by the section.

A rude shaft or pit was found to have been sunk through the beds of dufrenite into a mass of underlying limonite to a depth of ten or twelve feet. The irregular bed of dufrenite, made up of irregular nodular masses, having from one to eight inches of diameter, and incrustations of like varying thickness, lies near the surface of the ground, and has an average depth of ten or twelve inches, as far as could be determined in the presence of a considerable caving in of the old shaft.

This mineral had been thrown aside in mining as being of doubtful character, in the eyes of those who were exploring for iron ores, and several tons had been accumulated near the mouth of the opening; but since I first called attention to its true character, and although the locality is difficult of access, the whole of what was thrown out by the miners has been carried away and sent to different public institutions and to dealers in minerals.

This is, perhaps, the most extensive deposit of this mineral yet discovered in the United States.

Washington and Lee University, Lexington, Va., May, 1881.

ART. XIV.—*Turquois of New Mexico*; by B. SILLIMAN.*

THE existence of turquois, a comparatively rare gem, in New Mexico, is a fact long known. The chief locality is at Mt. Chalchuitl, in Los Cerillos, about twenty-two miles southwest of the ancient town of Santa Fé, the capital of that territory. We are indebted to Professor Wm. P. Blake for our first detailed notice of this ancient mine, in an article published in the American Journal of Science† in 1857.

It was subsequently visited by Dr. Newberry who mentioned it in one of his reports, and also by others. I have lately had an opportunity of examining this very interesting locality, since it has been laid open in the old workings and thus rendered accessible to observation by the recent explorations of Mr. D. C. Hyde.

The Cerillos Mountains have recently come into notice from the partial, and as yet superficial, exploration of very numerous mineral veins which are found to intersect them, and which

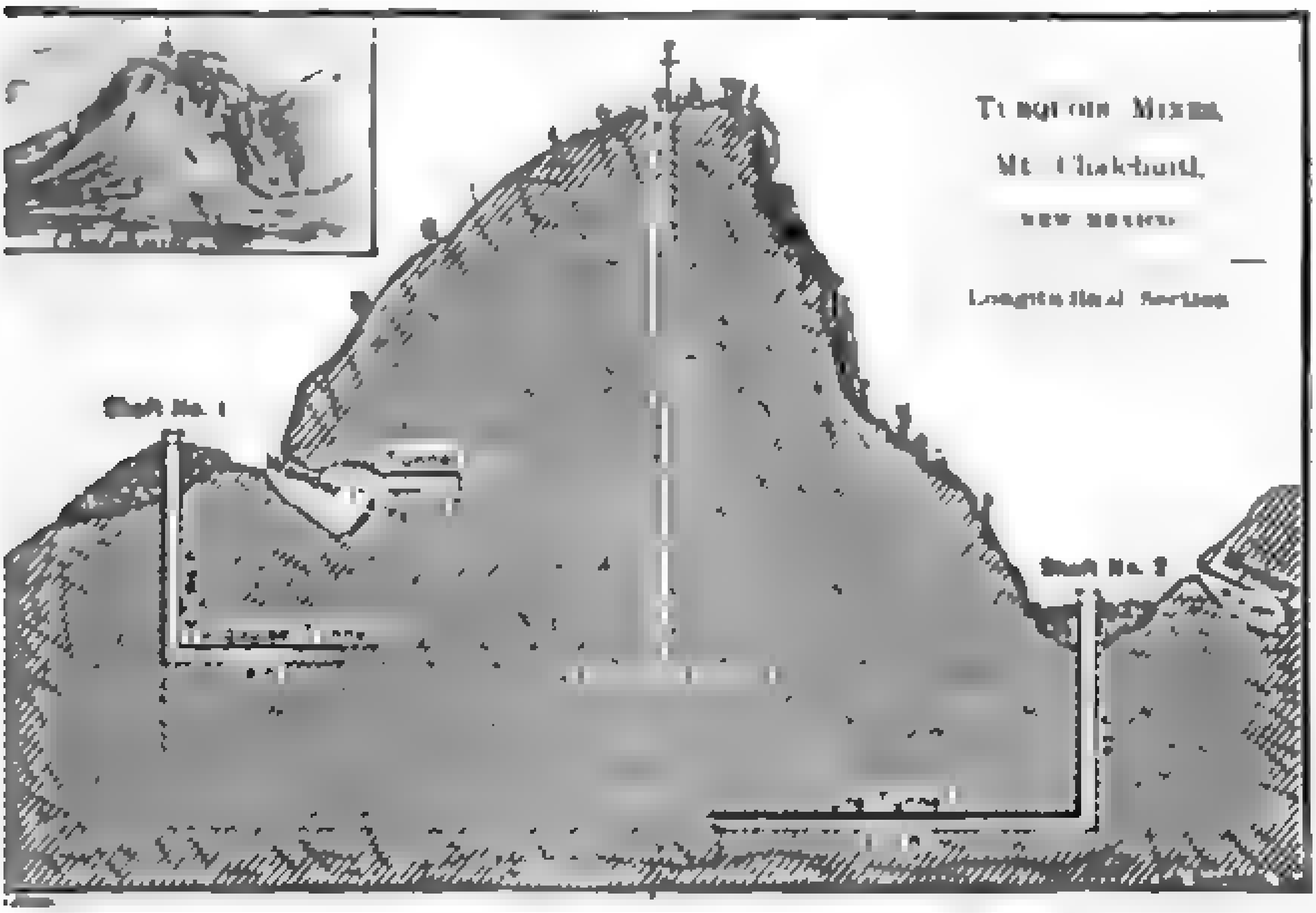
* Read before the American Association for the Advancement of Science, Boston, August, 1880.

† This Journal, 2d Ser., xxv, 27.

carry chiefly argentiferous galena, with some gray copper rich in silver, giving promise of mines of value when opened in depth. I have elsewhere spoken more particularly of these veins and of the rocks that contain them. These rocks are all eruptive rocks of the family of the augite trachytes, the kind which, the world over, carries the richest and most permanent ores of silver, with some gold. In the center of this district, which is not more than about six miles by four in extent, rises the dome of Mt. Chalchuitl (whose name the old Mexicans gave to the turquois, its much valued mineral), the summit of which is about 7,000 feet above tide, and is therefore almost exactly on a level with the Plaza of Santa Fé, across the valley of the river of that name, to the northeast. In the other direction this mountain has its drainage into the valley of the Galisteo, which forms the southern boundary of the Cerillos district. The age of eruption of these volcanic rocks is probably Tertiary. The rocks which form Mt. Chalchuitl are at once distinguished from those of the surrounding and associated ranges of the Cerillos by their white color and decomposed appearance, closely resembling tuff and kaolin, and giving evidence to the observer familiar with such phenomena of extensive and profound alteration; due, probably, to the escape through them, at this point, of heated vapor of water and perhaps of other vapors or gases, by the action of which the original crystalline structure of the mass has been completely decomposed or metamorphosed, with the production of new chemical compounds. Among these the turquois is the most conspicuous and important. In this yellowish-white and kaolin-like tufaceous rock the turquois is found in thin veinlets and little balls or concretions called "nuggets," covered with a crust of the nearly white tuff, which within consist generally, as seen on a cross fracture, of the less valued varieties of this gem, but occasionally afford fine sky-blue stones of higher value for ornamental purposes. Blue-green stains are seen in every direction among these decomposed rocks, but the turquois in masses of any commercial value is extremely rare, and many tons of the rock may be broken without finding a single stone which a jeweler, or virtuoso would value as a gem.

The observer is deeply impressed on inspecting this locality with the enormous amount of labor which in ancient times has been expended here. The waste or debris excavated in the former workings covers an area, which the local surveyor assured me extends by his measurement over at least twenty acres. On the slopes and sides of the great piles of rubbish are growing large cedars and pines, the age of which—judging from their size and slowness of growth in this very dry region

—must be reckoned by centuries. It is well known that in 1680 a large section of the mountain suddenly fell in from the undermining of the mass by the Indian miners, killing a considerable number, and that this accident was the immediate cause of the uprising of the Pueblos and the expulsion of the Spaniards in that year, just two centuries since.



The accompanying vertical section of the mountain from east to west will give a good idea of the old workings, and of the shafts and tunnels projected and partly carried out by Mr. Hyde. The irregular openings, named by Mr. Hyde "wonder caves" and the "mystery," are the work of the old miners, and the whole hillside from the flag-staff to the "mystery" was worked out by them also. It was this sharp slope of the mountain which fell. In these chambers, which have some extent of ramification, were found abundantly the fragments of their ancient pottery, with a few entire vessels, some of them of curious workmanship, ornamented in the style of color so familar in the Mexican pottery Associated with these were numerous stone hammers, some to be held in the hand and others swung as sledges, fashioned with wedge-shaped edges and a groove for a handle. A hammer weighing over twenty pounds was found while I was at the Cerillos, to which the wyth was still attached, with its oak handle—the same scrub oak which is found growing abundantly on the hillsides—now quite well preserved after at least two centuries of entombment in this perfectly dry rock.

The stone used for these hammers is the hard and tough hornblende andesite, or propylite, which forms the Cerro d'Oro and other Cerillos hills. With these rude tools and without iron and steel, using fire in place of explosives, these patient old workers managed to break down and remove the incredible masses of these tufaceous rocks which form the mounds already described.

That considerable quantities of the turquois were obtained can hardly be questioned. We know that the ancient Mexicans attached great value to this ornamental stone, as the Indians do to this day. The familiar tale of the gift of large and costly turquois by Montezuma to Cortez for the Spanish crown, as narrated by Clavigero in his history of Mexico, is evidence of this high estimation. It is not known that any other locality in America has furnished turquois in any quantity—the only other place thus far reported outside of Los Cerillos being that near Columbus District in Nevada, discovered by Mr. J. E. Clayton; and this is not yet worked.

The origin of the turquois of Los Cerillos in view of late observations is not doubtful. Chemically it is a hydrous aluminum phosphate. Its blue color is due to a variable quantity of copper oxide derived from associated rocks. I find that the Cerillos turquois contains 3·81 per cent of this metal. Neglecting this constituent, the formula for turquois requires: Phosphoric acid 32·6, alumina 47·0, water 20·5=100·?.

Evidently the decomposition of the feldspar of the trachyte furnishes the alumina, while the apatite, or phosphate of lime, which the microscope detects in thin sections of the Cerillos rock, furnished the phosphoric acid. A little copper ore is diffused as a constituent of the veins of this region, and hence the color which that metal imparts.

The inspection of thin sections of the turquois by the microscope, with a high power, detects that the seemingly homogeneous mass of this compact and non-crystalline mineral consists of very minute scales, nearly colorless, having an aggregate polarization, and showing a few particles of iron oxide.

The rocks in which the turquois occurs are seen, by the aid of the microscope and polarized light, in thin sections, to be plainly only the ruins, as it were, of crystalline trachytes; they show fragments of feldspar crystals, decomposed in part into a white kaolin-like substance, with mica, slag and glassy grains, and quartz with large fluidal enclosures, looking like a secondary product. There is considerable diversity in aspect, but they may all be classed as trachyte-tuffs and are doubtless merely the result of decomposition, as already indicated, of the crystalline rocks of the district along the line of volcanic fissures. In fact there are, in a northerly direction, other places,

one of them at Bonanza City, probably two or three miles distant, where the same evidence of decomposition is found, and in the rocks at this place I found also the turquois in forms not to be distinguished from those of the old mine. Mr. Hyde has shown me lately in New York a large number of the Cerillos turquois polished, one of huge size; and among them a few of good color and worthy of consideration as gems, some of them an inch in length and quite thick, but they are not of faultless beauty.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On Free Fluorine in Fluor Spar.*—The cause of the peculiar odor possessed by the dark violet fluor spar of Wölsendorf has been much discussed. Schafhäutl ascribed it to the presence of calcium hypochlorite, Schrötter to ozone, Schönbein to antozone, and Wyrouboff to a hydrocarbon. Loew, noticing the similarity of the odor on freshly fractured surfaces to that of chlorine, concluded that it was due to the presence of fluorine formed by the dissociation of some foreign fluoride present in minute quantity. The ozone theory was given up by Schrötter when he found that the odor was not destroyed by a heat of 310°. Moreover, he showed an alteration in this odor when the mineral was ground with potassium hydrate solution, and proved that an odor resembling that of sulphur chloride was produced when it was rubbed in a mortar with sulphur. Chlorine was separated from sodium chloride by it and iodine from potassium iodide. To test his fluorine hypothesis, Loew ground a kilogram of Wölsendorf fluor spar with water containing ammonia, using small portions at a time, the filtrate and wash-waters from the earlier being used with the later quantities. The last filtrate was mixed with sodium carbonate, evaporated, the residue treated in a platinum capsule with sulphuric acid, and, covered with a watch glass, kept at 40° to 50° for a long time. On examining the glass it was found to be very considerably corroded. Since fluor spar is not entirely insoluble in water, the experiment was repeated, using the inodorous mineral. The result was so exceedingly feeble as to dispose entirely of this objection to the former result. Since these dark radiated varieties of fluorite contain cerium, the author thinks a ceric fluoride is the source of the free fluorine, by dissociating into cerous fluoride and fluorine, analogous to the decomposition of manganese tetra-chloride at ordinary temperatures.—*Ber. Berl. Chem. Ges.*, xiv, 1144, May, 1881.
G. F. B.

2. *On Arsenobenzene.*—Azo-benzene $C_6H_5N{=}NC_6H_5$, has long been known, and phosphobenzene $C_6H_5P{=}PC_6H_5$ has recently been discovered. The corresponding compound of arsenic, arseno-

benzene $C_6H_5As{=}AsC_6H_5$ has now been obtained by MICHAELIS and SCHULTZE. For this purpose phenyl-arsenous oxide C_6H_5AsO was acted upon in alcoholic solution by reducing agents, preferably phosphorous acid. No change takes place in the cold but on heating nearly to boiling the reaction takes place and the mass solidifies in crystals. On filtering, washing with hot alcohol and drying in a vacuum over sulphuric acid, the arsenobenzene is obtained pure, in the form of pale yellow needles, difficultly soluble in alcohol, insoluble in water and ether. Chloroform, carbon disulphide, and benzene dissolve it easily, but the solution resinifies readily. Beautiful crystals are obtained on cooling from solution in hot xylene. Chlorine unites with it directly to form phenyl-arsenous chloride. It fuses at 196° to a yellow liquid, and decomposes above this, evolving triphenylarsine and metallic arsenic. Phenyl-arsenous iodide when reduced gives arseno-iodo-benzene $C_6H_5IAs - AsIC_6H_5$. Naphthalene acts similarly, an arsenonaphthalene $C_{10}H_7As{=}AsC_{10}H_7$ being produced by the reduction of naphthyl-arsenous oxide by phosphorous acid.—*Ber. Berl. Chem. Ges.* xiv, 912, Apr., 1881. G. F. B.

3. *On the Transformation of Dextrose into Dextrin.*—Some years ago, MUSCULUS observed that when dextrose was dissolved in concentrated sulphuric acid, a new body was obtained which was probably a dextrin. The recent experiments of Gautier, have led Musculus in conjunction with MEYER, to re-examine this subject. Twenty grams of pure dextrose was melted in a calcium-chloride bath; after cooling thirty grams of concentrated sulphuric acid was added in four or five successive portions, the whole being stirred with a thermometer, the temperature being allowed to rise to 60° and the mixture to become brown. Eight hundred parts of absolute alcohol were then added, the solution filtered and allowed to stand for eight days. The abundant precipitate was collected on a filter and washed, first with cold and then with boiling absolute alcohol till all traces of acid were removed. It was then dried. In this way there were obtained ten grams—half the dextrose used—of a perfectly white amorphous powder, hygroscopic but not deliquescent. It contains alcohol not removable by drying over sulphuric acid for months or by a heat of 100°. By solution in water and distillation 9·3 per cent of alcohol was obtained. Heated to 110°, the alcohol evaporates and the remaining powder is extremely deliquescent. On analysis it gave numbers agreeing with the formula $C_{18}H_{28}O_{14}$. Hence the first powder was a combination of this with a molecule of alcohol, $C_{18}H_{28}O_{14}.C_2H_6O$, which requires 8·9 per cent of alcohol. This, when decomposed by water, the alcohol removed by evaporation and the residue dried over sulphuric acid, gives a body whose analysis agrees with the formula $C_6H_{10}O_5$. When therefore the alcohol in the above formula is replaced by water the formula becomes $C_{18}H_{28}O_{14}.H_2O$ or $(C_6H_{10}O_5)_3$. This hydrated body possesses all the physical, chemical and organoleptic properties of a dextrin. It is amorphous, yellowish, very soluble in water, of a

flat sweetish taste, is not colored by iodine, is precipitated by alcohol from its aqueous solution, reduces only very feebly Fehling's test, rotates to the right the plane of polarized light $[\alpha]=+131$ to $+134°$, does not ferment with yeast, is not saccharified by diastase, is converted into dextrose by prolonged boiling with dilute sulphuric acid, and has the diffusibility of a dextrin, being nearest to the γ-dextrin of Musculus.—*Bull. Soc. Ch.*, II, xxxv, 368, Apr., 1881. G. F. B.

4. *On Pentathionic Acid.*—LEWES has satisfactorily established the existence of pentathionic acid. Continuous currents of hydrogen sulphide and sulphurous oxide gases were passed, according to Wackenroder's method, into distilled water, the former in slight excess, for seven hours, the mass heated on a water bath, filtered from sulphur and analyzed. Three separate methods gave in 10 c.c. 0·23, 0·227 and 0·226 of sulphur. On titrition, 1 c.c. neutralized 0·01457 gram K_2O, equal to 0·012 gram potassium; thus giving 2 : 4·55 for the ratio of K : S, and suggesting the presence of an acid having more sulphur than the tetrathionate. Having noticed that a partly neutralized solution decomposed only very slightly, Lewes added to a solution prepared as above, a weak solution of barium hydrate, sufficient to neutralize only half of it. On filtering after standing twenty-four hours, a clear solution was obtained which was placed in a vacuum over sulphuric acid. After 18 days a crop of fine needle-shaped crystals was obtained which proved on analysis to be barium tetrathionate. In a few days a second crop of crystals was obtained consisting of thin square plates mixed with a few oblong rectangular crystals, which gave on analysis numbers between those of tetra- and pentathionate, probably a double salt. A third crop of very small oblong rectangular crystals was obtained which gave on analysis numbers agreeing with the formula $BaS_5O_6(H_2O)_3$. The salt is soluble in cold water and if not too concentrated the solution may be boiled. The reactions of the solution are given. By the same process, three potassium salts of pentathionic acid were obtained; one in semi-opake, probably rhombic crystals $K_2S_5O_6(H_2O)_2$; another in small and apparently monoclinic crystals, having one molecule of water of crystallization; and a third in very small, short prisms, which is the anhydrous pentathionate $K_2S_5O_6$. These salts may be easily prepared as they are much more stable than the barium salt. They are distinguished from the corresponding tetrathionates by the fact that they give an immediate precipitate of sulphur on adding an alkali hydrate.—*J. Chem. Soc.*, xxxix, 68, March, 1881. G. F. B.

5. *Photographics: A Series of Lessons, accompanied by Notes, on all the Processes which are needful in the Art of Photography;* by EDW. L. WILSON. 8vo, pp. 352. Philadelphia, 1881.—Mr. Wilson has sought in this book to produce a hand-book for the professional as well as for the amateur photographer. The plan is somewhat novel. After giving in a clear and satisfactory way, on the upper half of the page, the matter culled from his own

experience, he prints in smaller type, on the lower half, quotations bearing directly on the subject in hand, and taken from the best authorities known. In this way the opinions of over two hundred authors have been secured to the reader. The science and the art of photography is given in twenty-seven lessons, each treating of one branch. The first of these on the treatment of the subject is an excellent discussion of the esthetic in photography, illustrated from the masters in art. Then follows the technique of the wet plate process in all its parts. The dry plate process follows this, and then some of the more recent phototype processes, and the book closes with some useful practical suggestions. The work appears to be a great success in its manner as well as its matter. It will certainly become the standard book on photography in this country. G. F. B.

6. *Conservation of Electricity.*—In a memoir by M. G. LIPPMANN, presented to the French Academy by M. Jamin, the author maintains that the quantity of matter and the quantity of energy are not the only magnitudes in nature which remain invariable; the quantity of electricity in the universe is also invariable. The distribution of electricity can change, but the quantity of free electricity never varies. The sum of the quantities of free electricity is invariable since the total variations of the charges is always equal to zero. Let x and y be two independent variables upon which the quantity of electricity which a body receives depends; x can be, for example, the potential which the body acquires, y its capacity, or a quantity proportional to the capacity. Let dm be the quantity of electricity received by a body when x is increased by dx and y by dy; one can then write $dm = Pdx + Qdy$, in which P and Q are two functions of x and of y. The principle of the conservation of electricity is expressed by the condition that dm shall be an exact differential. Divide, for instance, any system in which an electrical phenomenon is produced, into two portions, A and B. Let a and b be the simultaneous variations of these two portions. In virtue of this principle of the Conservation of Electricity, we must have $a+b=0$. When A passes over a closed cycle, that is to say, when its final state corresponds to its initial one, $a=0$ and $b=0$. We can then write $\int dm = 0$. In order that $\int dm$ may be zero for every closed cycle, it is necessary that dm shall be an exact differental, or $\frac{dP}{dy} = \frac{dQ}{dx}$. In this manner we can write the analytical expressions for the general principle of the Conservation of Electricity.—*Comptes Rendus*, No. 18, May 2, 1881. J. T.

7. *Inverse Electromotive force of the Voltaic arc.*—M. J. Jamin corroborates the statements of M. Le Roux in regard to the inverse electromotive force which arises from the carbon points of the electric lights. This electromotive force is nearly equivalent to that of ten to fifteen Bunsen elements. In obtaining, therefore, a light from a battery of thirty to forty Bunsen cells,

only twenty-five are useful in maintaining the light. Thus it is difficult to produce two or a greater number of arcs in the same continuous current, since it is necessary to overcome the inverse electromotive force of each light. This fact is an objection to the use of batteries, continuous current machines, secondary batteries like those of Planté or of Faure. The conditions, however, are very different with the use of alternate current dynamo-electric machines; for with a certain speed of alternation the effect of the inverse electromotive force is a minimum. The difference of temperature of the carbon points determines the strength of the inverse electromotive force, and when this difference of temperature tends to disappear as it does when alternate currents are employed, the inverse electromotive force is very much diminished.—*Comptes Rendus*, No. 18, May 2, 1881. J. T.

8. *Stellar Photography.*—In a letter addressed to M. A. Cornu, H. DRAPER relates that he has succeeded in photographing, after an exposure of one hundred and forty minutes, the stars in the nebula of Orion, which can be represented in size by the numbers 14·1, 14·2, 14·7, according to the scale of Poyson. Photography has thus secured images of stars nearly at the limit of visibility in a telescope of nine inches aperture. It seems, therefore, not improbable that stars which are invisible to the eye in a telescope of this size can be photographed.—*Comptes Rendus*, No. 16, April, 1881. J. T.

9. *Weather Warnings.*—Professor BALFOUR STEWART, in a lecture delivered at South Kensington, April 29, spoke of the probability that British magnetical weather may be followed after five or six days by corresponding meteorological weather. From a preliminary trial, Professor Stewart believes that it may be possible to forecast meteorological weather some five or six days by means of the variations of the magnetic elements.—*Nature*, May 5, 1881. J. T.

10. *Storing of Electricity.*—M. FAURE has modified the secondary battery of Planté by coating the lead plates with a covering of minium. The sheets of lead are separately covered with minium and rolled together in a spiral with a layer of felt between, and are then placed in a vessel of sulphuric acid and water. When a current is passed into this cell the minium on one plate is reduced to metallic lead and on the other is oxidised to peroxide. When the cell is discharged this action is reversed. According to M. Reynier, one of these spiral cells weighing 75 kilograms can store up energy sufficient to furnish one horse power for an hour.—*Nature*, May 19, 1881. J. T.

II. GEOLOGY AND NATURAL HISTORY.

1. *Sketch of the Geology of British Columbia;* by GEORGE M. DAWSON, D.S., A.R.S.M., F.G.S.—British Columbia includes a certain portion of the length of the Cordillera region of the west coast of America, which may be described as consisting here of

four parallel mountain ranges running in a northwest and southeast bearing. Of these the southwestern is represented by Vancouver and the Queen Charlotte Islands, and may be referred to as the Vancouver Range; while the next, to the northeast, is the Coast or Cascade Range, a belt of mountainous country about 100 miles in width. This is succeeded by the interior plateau of British Columbia, relatively a depressed area, but with a height of 3000 to 3500 feet. To the northeast of this is the Golden Range, and beyond this the Rocky Mountains proper, forming the western margin of the great plains of the interior of the continent.

Tertiary rocks, which are probably of Miocene age, are found both on the coast and over the interior plateau. They consist on the coast of marine beds, generally littoral in character, which are capped, in the Queen Charlotte Islands, by volcanic rocks. The interior plateau has been a freshwater lake, in or on the margin of which, clays and sandstones, with occasional lignites, have been laid down. These are covered with very extensive volcanic accumulations, basaltic or tufaceous.

Cretaceous rocks from the age of the Upper and Lower Chalk to the Upper Neocomian, and representing the Chico and Shasta groups of California, occur on Vancouver and the Queen Charlotte Islands. Beds equivalent to the Chico group yield the bituminous coals of Nanaimo, while anthracite occurs in the somewhat older beds of the Queen Charlotte Islands. Within the Coast range the Cretaceous rocks are probably for the most part equivalent in age to the Upper Neocomian. The Cretaceous rocks are of great thickness, both on the coast and inland, and include extensive contemporaneous volcanic beds.

The pre-Cretaceous beds had been much disturbed and altered before the deposition of the Cretaceous, and their investigation is difficult. On Vancouver Island, beds probably Carboniferous in age include great masses of contemporaneous volcanic material, with limestones, and become altered to highly crystalline rocks resembling those parts of the Huronian of Eastern Canada. In the Queen Charlotte Islands these beds also probably occur; but an extensive calcareous argillite formation is there found, which is characterised by its fossils as Triassic.

The Coast Range is supposed to be built up chiefly of rocks like those of Vancouver Island, but still more highly altered, and appearing as gneisses, mica-shists, &c., while a persistent argillaceous and slaty zone is supposed to represent the Triassic argillites of the Queen Charlotte Islands.

The older rocks of the interior plateau are largely composed of quartzites and limestones; but still hold much contemporaneous volcanic matter, together with serpentine. Carboniferous fossils have been found in the limestones in a number of places. The Triassic is also represented in some places by great contemporaneous volcanic deposits with limestones.

In the Golden Range, the conditions found in the Coast Range are supposed to be repeated; but it is probable that there are

here also extensive areas of Archæan rocks. Some small areas of ancient crystalline rocks, supposed to be of this age, have already been discovered.

The Rocky Mountain Range consists of limestones with quartzites and shaly beds, dolomites and red sandstones. The latter have been observed near the 49th parallel, and are supposed to be Triassic in age. The limestones are, for the most part, Carboniferous and Devonian, and no fossils have yet been discovered indicating a greater age than the last-named period. On the 49th parallel, however, the series is supposed to extend down to the Cambrian, and compares closely with the sections of the region east of the Wahsatch, on the 40th parallel, given by Clarence King. Volcanic material is still present in the Carboniferous rocks on the 49th parallel.

The oldest land is that of the Golden Range, and the Carboniferous deposits laid down east and west of this barrier differ widely in character. The Carboniferous *closed with a disturbance* which shut the sea out from a great area east of the Gold Range, in which the red gypsiferous and saline beds of the Jura-trias were formed. In the Peace River region, however, marine Triassic beds are found on both sides of the Rocky Mountains.

A great disturbance, producing the Sierra Nevada and Vancouver ranges, closed the Triassic and Jurassic period. The shore line of the Pacific of the Cretaceous in British Columbia lay east of the Coast Range, and the sea communicated by the Peace River region with the Cretaceous Mediterranean of the great plains.

No Eocene deposits have been found in the province. The Miocene of the interior plateau is probably homologous with King's Pah-Ute lake of the 40th parallel Miocene. In the Pliocene the country appears to have stood higher above the sea-level than at present, and during this time the fiords of the coast were probably worn out.—*Proc. Geol. Soc. London*, 1881.

2. *Caribbean Miocene fossils.*—A memoir, on Miocene fossils of Sapote, Costa Rica, and a few from Gatun, on the Panama Railroad, by the late W. M. Gabb, is published in Part IV of vol. viii (2d Ser.) of the Journal of the Academy of Natural Sciences. A number of the species are identical with Miocene species of San Domingo.

3. *Report of the State Geologist of New Jersey for the year* 1880.—Professor GEORGE H. COOK, the State Geologist, devoted a considerable part of his last report to a discussion of the relations of the soils of the various regions of the State to the accompanying rocks, which subject was illustrated by a colored map of the State. The Report for 1880 contains an extended account of the Glacial drift over New Jersey, including the facts as to the course of the terminal moraine across the State, terraces along valleys, and those as to other gravel and sand deposits, chiefly in Southern New Jersey, which are regarded as of pre-glacial origin.

He then shows that on the Passaic River, southwest of Patterson, the waters of the flooded river were spread into a lake 30 by 6 or 8 miles in its diameters and 200 feet deep, owing to the confining ridges of trap on the east and south. One of the most remarkable bowlder deposits in the southern extremity of the State is in Cape May County about Dennisville, especially between Dennis Creek and Cumberland County. The bowlders have worn but not rounded edges, and have not been observed to have glacial markings. The largest, in North Dennisville, measured 14 feet in length and averaged 11 by 17 inches in its other dimensions; another is 7 feet in diameter. It is suggested that they may have come on floating ice down the Delaware when the waters stood 60 feet above their present level.

At Paterson a well has been sunk 2100 feet in the Red Sandstone (Triassic), proving thus that the thickness of the rock exceeds this amount. It obtained water at 1120 and 2050 feet; and that at the latter depth (which ascended to within 30 feet of the surface) was saline, it containing about half as much common salt as the water of the ocean, and more of chlorides of potassium, calcium and magnesium. The total amount of solid matter per gallon was 929·46 grains.

4. *Geological Survey of Pennsylvania.*—The legislature of Pennsylvania has passed, and the Governor has approved, a bill appropriating $125,000 to the State Geological Survey under the direction of Professor J. P. Lesley. This insures the completion of this great work in 1883, ten years from its commencement, the whole expense having been $445,000, besides the printing.

The Geology of the Oil Regions of Warren, Venango, Clarion and Butler Counties; by JOHN F. CARLL, Report III' of the Geological Survey of Pennsylvania. 482 pp., 8vo.—Mr. Carll's report shows careful and judicious observation in all its chapters, whether treating of geology or the characteristics of the oil-producing regions; the condition of the oil deposits, the origin of the oil and of the associated beds; or of the topography, drainage, and drift phenomena of the districts. In addition, it gives an account of oil-well exploration, machinery and tools. In these and all its subjects, it is well illustrated by drawings and sections. It is a work of great practical and scientific value.

5. *Annual Report of the Bureau of Statistics and Geology of Indiana for* 1880.—In Indiana, the duties of State Geologist were, in 1879, transferred to the Bureau of Statistics and Geology, of which Professor John Collett, an excellent geologist, is the Chief. It is creditable to the intelligence of that State, that their law requires that the head of that Bureau shall be an expert in the sciences of geology and chemistry. Professor Collett has published two annual reports, the last of which contains about fifty pages on geology with plates of fossils. J. M.

6. *Illustrations of the Earth's Surface: Glaciers;* by N. S. SHALER, Professor of Palæontology, and WM. M. DAVIS, Instruc-

tor in Geology, in Harvard University; 196 pp., large 4to, with 25 plates. Boston, 1881. (James R. Osgood & Co.)—The plan of the series of which this volume is the first is to present illustrations of prominent subjects in geology—Glaciers, Mountains, Volcanoes, Earthquakes, etc., as far as possible from photographs, and accompanying text giving "a connected idea of the more essential facts and theories that belong to each subject." The volume which has been issued, on Glaciers, is exceedingly well adapted for its purpose. Its illustrations represent some of the most characteristic of glaciers, with a degree of perfection scarcely exceeded by the photograph, and on a scale of magnitude, owing to the large 4to size, that exhibits all details in perfection. Among them are the Glacier des Bossons, de Talèfre from the Jardin, the Aletsch in several views, du Géant, and others, in the Alps, with some from the Himalaya, Norway, etc. Besides these, several plates are devoted to other Glacial phenomena, and some to those of the Glacial era, especially the American. The subjects are happily chosen for instructiveness, and the beauty of the plates is remarkable. The text gives an excellent general review of the subject of glaciers modern and ancient, with many important descriptive details. It discusses Croll's theory of the origin of glacial cold, with criticisms, and also other opinions on the subject; treats of the movement of glaciers; of glacier deposits; of soils from glaciers; of the blue and yellow clays—attributing the latter to oxidation since deposition, as done by Van den Brœck in the work mentioned beyond. The volume is a very valuable one for both instructor and student.

7. *The Trilobite: New and Old Evidence Relating to its Organization;* by C. D. WALCOTT. Bull. Comp. Zool., vol. viii, No. 10. —Mr. Walcott here presents the results of his remarkable dissections of Trilobites, with full illustrations on six plates. The species examined were *Ceraurus pleurexanthemus*, *Calymene senaria*, and *Asaphus platycephalus.* The results show, beyond question, the existence of a series of jointed organs about the mouth, and appear to indicate a continued series down the thorax and into the pygidium, besides exhibiting remains of ambiguous organs, looking as if spiral, and supposed by the author to be branchial in relations. A "restoration" of *Calymene senaria* is given on plate vi. The series of legs in this restoration looks very doubtful, for, if so distinct in the animal, it seems to be incomprehensible that such dissections should have been needed for their discovery. A series of distinct ambulatory legs on a large Trilobite should have been large and stout, and could hardly have escaped preservation in the form of large and stout limbs. It may be that the supposed joints of the legs of the thorax and posterior extremity, which have the appearance of having been thin or membranous, are merely subdivided and thickened portions of the outer ventral shell, which served as attachments for thin membranous articulated appendages such as have hitherto been attributed to Trilobites. J. D. D.

8. *Geological Survey of Alabama: Report of progress for 1879 and 1880*; by EUGENE A. SMITH, Ph.D., State Geologist; 158 pp., 8vo.—This report contains a detailed description of the coal-measures of the Warrior Coal Field, and is accompanied by a geological map of the region.

9. *The Felsites and their associated rocks north of Boston;* by J. S. DILLER. Bulletin of the Museum of Comparative Zoology at Harvard College, vol. vii, (Geological Series, vol. i, pp. 165 to 180, 8vo).—Prof. Diller treats of the physical and other characters of the felsitic rocks, including felsites and conglomerates, of Medford, Malden, Melrose, Wakefield, Saugus and Lynn, in Eastern Massachusetts, and of some of the adjoining rocks. He arrives at the conclusion that the felsites are eruptive rocks. He gives for the order of age for the rocks referred to as eruptive: granite, felsyte, dioryte, and diabase and melaphyre.

10. *Mémoire sur les Phénomènes d'Altération des Dépots superficiels par l'infiltration des eaux Météoriques, étudiés dans leurs rapports avec la Géologie stratigraphique*, par ERNEST VAN DEN BRŒCK, Conservateur au Musée Royale d'Histoire Naturelle, Attaché au service de la Carte Géologique. 180 pp. 4to, with a folded plate. Bruxelles, 1881. From vol. xliv of Mém. Couronnés et Mém. des Sav. Etr. of the Brussels Academy.—The facts and conclusions in this important memoir sweep away much that is erroneous in Quaternary stratigraphical geology. The principle appealed to is one well understood—that iron-oxidation and other metamorphic changes are carried downward into deposits, consolidated or not, by infiltrating waters; but the extent of the changes thus occasioned has not been so well appreciated. On this point the observations of the author throw much light. The diluvial deposits of many parts of western Europe have been described as consisting of "diluvium gris" below and "diluvium rouge" above; and the distinction has seemed to be of special importance by many recent writers. The author shows, and illustrates his facts by many sections, that the red beds are the gray beds turned red by oxidation through infiltering waters. His sections represent downward prolongations of the red into the gray, and layers of gravel of the gray beds continuously through the red without interruption or disturbance. In other cases gray beds are overlaid by yellow beds or gray clays by yellow clay deposits; and as before, the upper yellow bed is not a distinct bed, but a result of the superficial alteration of the gray through infiltrating waters producing oxidation. The large plate contains a number of colored sections of Quaternary deposits, fully sustaining his conclusions.

11. *On the application of a solution of mercuric potassium-iodide in mineralogical and lithological investigations*, by V. GOLDSCHMIDT.—The ingenious method for separating mechanically the mineral constituents of a rock, proposed by M. Thoulet, has already been extensively employed by lithologists. This method is based upon the fact that a solution of mercuric iodide and potassium iodide in water may be obtained having a very

high specific gravity; and further that, by the addition of distilled water drop by drop, any required density, from the maximum (Thoulet) 2·77, down to 1, may be obtained. If now the fine fragments of a rock be introduced into the solution, those whose density is equal or less than that of the solution will float and all others will sink. By carrying on the process in a suitable vessel and by varying, as circumstances require, the density of the menstruum, the separation of several different minerals may be accomplished. For the further discussion of the subject, as given by M. Thoulet, reference must be made to his valuable memoir on "Contributions a l'étude des propriétés physiques et chimiques des minéraux microscopiques;" Paris, 1880 (also Bull. Soc. Min. France, ii, 17, 1881). This method has been exhaustively studied by Goldschmidt, and the results given in his memoir show how much can be accomplished in this way that was impossible by any of the earlier methods of mechanical separation; at the same time he calls attention to the conditions upon which success depends and to the various opportunities of error. The maximum density obtained by him was 3·196 but varying somewhat with the temperature. By the use of the solution Goldschmidt shows that with due care the specific gravity of a pure mineral in fragments can be obtained with an error of only 2 or 3 units in the fourth place of decimals. He determines in this way the specific gravity of a large series of specimens of different kinds of feldspar and concludes that the method gives a sure means of separating the different species of the group when fresh and pure. In regard to the best manner of separating the constituents of a rock the author gives many practical hints of value, and details the results obtained by him in a number of typical cases. It has also been proposed to use this solution of mercuric potassium iodide for various optical purposes as that of determining the indices of refraction by total reflection by the method of Kohlrausch. Goldschmidt finds the maximum index of refraction to be 1·73 (for D) and he has investigated the variation in refractive index for solutions of different strengths.—*Jahrb. Min.*, 1881.

12. *Deer horns impregnated with tin ore.*—Mr. J. H. Collins describes, in the Transactions of the Royal Geological Society of Cornwall, deer-horns, now in the British Museum, that were found in the tin-bearing gravel of the Carwon and Pentewan Valleys, which are impregnated with tin-ore, and seem to have, "in some parts, the original horn structure almost entirely preserved or reproduced in oxide of tin" and even contain in places visible crystals of this oxide. They are reported as having been formerly common and as having been sold as block tin to the smelters. These specimens have not been analyzed; but a fragment, belonging to the Cornwall Geological Society, which appears to be part of the horn of a *Cervus elaphus* (the red deer) afforded him on analysis 2·60 per cent. of stannic oxide, and 1·66 of iron sulphide; and, although the amount of these introduced ingredients is small, they were found, on microscopic

examination, to be distributed in the interior of each cell through the mass. Mr. Collins supposes that the tin was introduced by means of the fluoride.

13. *Microlite from Amelia County, Virginia.*—The rare species microlite, hitherto known only in minute crystals from Chesterfield, Mass., Branchville, Ct., and Utö, Sweden, has been recently found by Prof. W. M. Fontaine in Amelia County, Va., and is described by Prof. F. P. Dunnington. It occurs in isolated octahedral crystals from $\frac{1}{10}$ to $\frac{3}{4}$ inches in diameter and in larger crystalline masses, one of which weighed *eight pounds.* The physical characters are: H.=6 or a little less; G.=5·656; luster glistening resinous; color, wax yellow to brown; streak, pale ochreous yellow; sub-translucent; fracture conchoidal; very brittle. An analysis gave:—

Ta_2O_5	Cb_2O_5	WO_3	SnO_2	CaO	MgO	BeO	U_2O_3	Y_2O_3
68·43	7·74	0·30	1·05	11·80	1·01	0·34	1·59	0·23

Ce_2O_3, Di_2O_3	Al_2O_3	Fe_2O_3	Na_2O	K_2O	F	H_2O, deduct
0·17	0·13	0·29	2·86	0·29	2·85	1.17

O replaced by F
1·20 = 99·05

This shows the mineral to be essentially a calcium pyrotantalate. The formula deduced is— $\left\{ \begin{matrix} 3(Ca_2Ta_2O_7) \\ (mCaWO_3) \end{matrix} \right. + CbOF_3$.
—*Amer. Chem. Journal,* iii, 130, May, 1881.

14. *Mya arenaria.*—A paper in the American Naturalist for May last, by R. E. C. Stearns, reports that this mollusk, the "long clam" of eastern waters, has recently become the "leading clam" in the markets of San Francisco and Oakland, although unknown on the coast until the discovery of a few specimens on the eastern side of Francisco bay in 1874. How introduced is yet an unanswered question.

15. *Rhizopods, the food of some young Fishes.*—Dr. Leidy reports that the young of some of the suckers (Catostomidæ), *Hypentelium, Myxostema,* etc., have been found by Mr. S. A. Forbes, of Illinois, to have the intestines packed with tests of Difflugia and Arcella, indicating that they feed on Rhizopods. In a slide containing material from the intestines of the young Mullet (*Myxostoma macrolepidotum*) from Mackinaw Creek, prepared by Mr. Forbes, Dr. Leidy distinguished *Difflugia globulosa* and D. *acuminata;* and in another of the food of *Eremyzon succetta* he found *Difflugia globulosa, D. lobostoma, D. pyriformis, Arcella vulgaris, A. discoides,* besides another peculiar undescribed form.—*Proc. Acad. Nat. Sci. Phila.,* Jan. 4, 1881.

III. Astronomy.

1. *On the Figures of the Planets.*—The conclusions of Professor Hennessy in regard to the form of the planet Mars have been given on p. 162 of the last volume of this Journal (Feb., 1881). In a recent paper in the *Comptes Rendus* (1881, p. 225)

he gives the formulas deduced by him for the compression (e) of a planet resulting from superficial abrasion, and shows that this would be sensibly less than that resulting from the hypothesis of primitive fluidity. The application of the formulæ to the planets whose times of rotation and mean density are most similar to the earth give the following results :—

For the planet Mercury, if we admit 86700″ for its time of rotation, ·075 for the ratio of its mass to that of the earth, and ·378 for the ratio of its diameter to the earth's mean diameter, we find $Q = \frac{1}{406 \cdot 3}$; and if the planet were homogeneous,

$$e = \frac{1}{325}.$$

With the same law of density as in the earth, on the fluid theory,

$$e = \frac{1}{413};$$

and on the theory of abrasion,

$$e = \frac{1}{586}.$$

These three results show that for Mercury no sensible compression is likely to be observed.

For Venus, if we adopt the values of the mass M, time of rotation T, and diameter a, generally admitted, namely

$$M = \frac{1}{412150}, \quad T = 23^h\ 21^m\ 22^s, \quad a = \cdot 954,$$

I find for the compression, on the hypothesis of fluidity and a law of density like that for the earth,

$$e = \frac{1}{247},$$

and by the hypothesis of abrasion at surface,

$$e = \frac{1}{351}.$$

The first of these values approaches closely to the compression recently observed by Colonel Tennant—namely, $e = \frac{1}{260}$. So far, therefore, the figure of Venus is more consistent with the theory of fluidity than with the theory of superficial abrasion.

Since I communicated my note on Mars to the Academy, I have become acquainted with the new determination of the planet's mass obtained from the motions of its satellites. The astronomers of the Washington Observatory have devoted especial attention to the satellites of this planet. Professor Asaph Hall has published results* which lead to the conclusion that the mass of Mars is probably about $\frac{1}{3093500}$.

With this value, and the values of other elements remaining

* Washington Astronomical Observations, xxii, Appendix.

the same as in my previous note, Q becomes $\frac{1}{203 \cdot 74}$ or $\frac{1}{204}$ nearly. The compression on the fluid theory becomes $\frac{1}{206 \cdot 96}$ or $\frac{1}{207}$. On the theory of abrasion the compression is $\frac{1}{303}$. The first is much nearer to the observed compression $\frac{1}{219}$ than the last.

It thus appears that, for the earth and the planets nearest to it, and whose mean density and general appearance make it probable that their materials resemble those of the earth in physical and mechanical properties, the compressions deduced from the theory of fluidity agree much better with observation than the compressions deduced from the theory of superficial abrasion.—*Phil. Mag.*, April, 1881.

2. *Observations of the Transit of Venus*, Dec. 8–9, 1874. Part I. Washington: Government Printing Office. 1880. Edited by SIMON NEWCOMB.—This is the first of four proposed parts in which the *Observations* made and reduced under the direction of the Commission created by Congress are to be published. The remaining parts will give the observations in detail, the discussion of the longitudes of the stations and the measures of the photographs with their reduction and discussion. The present part gives the general account of the operations and the reduction and results of the observations, and logically might have been the last instead of the first part.

The most important chapters are the third and fourth. The discussion of photographic instruments and measurements, and the formation of the observation equations fill the third chapter. There were over 200 photographs which could be measured, furnishing over 400 observation equations for determining the most probable corrections to the tabular place of the planet and the assumed solar parallax.

The discussion of the errors and discrepancies among the photographic results, and the determination of a value of the solar parallax are not given, as the Astronomische Gesellschaft has discouraged the publication of separate results for the solar parallax until the whole of the observations of all parties can be combined in a single discussion. The remark is made, however, that the probable error of the photographic measurements far exceeds what was originally estimated.

The fourth chapter gives a treatment of the contact observations, of which twenty-five were secured. These, also, are reduced to the form of observation equations, very like those from the photographs.

The lessons which these results furnish with reference to the observations of the transit in 1882 are not developed, but it seems probable that the photographic methods must be improved, or else not made our principal reliance in the coming transit. H. A. N.

3. *Observations of Double Stars made at the U. S. Naval Observatory;* by ASAPH HALL.—Professor Hall has given in

this memoir the results of his observations on double stars with the twenty-six inch equatorial of the Naval Observatory, made during the five years, 1875–9, together with a few measures made in 1863, with the 9.6 equatorial.

A group of observations is first given on selected stars, made in concert with Mr. Struve and Baron Dembowski, for the purpose of eliminating constant errors of position angle if possible.

A series of measures upon two triple stars and upon the trapezium of Orion, give further means of estimating the accuracy of Professor Hall's measures with the great equatorial. The main part of the memoir is devoted to the measures of other double stars. The total number of observations is 1,614 on over 400 different stars. When we consider that one good observation of a double star is worth scores of those of moderate or doubtful value, we appreciate more highly the value of such a series of observations by such an observer. H. A. N.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Historical Sketch of the Boston Society of Natural History, with a notice of the Linnæan Society which preceded it;* by THOMAS T. BOUVÉ. 250 pp. 4to, with several portraits. From the Anniversary Memoirs of the Boston Society of Natural History, published in celebration of the Fiftieth Anniversary of the Society's foundation.—This volume comprises an important part of the history of American science. The Linnæan Society, which was the predecessor of the Natural History Society, was begun in 1814, at first under the name of The New England Society of Natural History, but a month later, that of the Linnæan Society of New England, and in 1823 its last meeting was held. When the Boston Society of Natural History commenced, in 1830, it acquired possession of what remained of the collections of the Linnæan Society, but "nothing of any considerable value was obtained." The society was without endowment, and the income for the first year from the fees of members and a course of lectures, after deducting the expenses of the lectures, was but little over five hundred dollars. Through the liberality of its friends, it now has a fund of more than $150,000, a building that cost as much as this, a large library, extensive collections, and many volumes of its own published Memoirs and Proceedings. Considering the expenses of publication, of the care of specimens, the great importance of extending the collections, and the required outlays for curators, librarian, and other urgent needs, the amount is still small; and yet that it is so much is an honor to the generous citizens of Boston, who are sure to keep making it larger. Mr. Bouvé, in his excellent history of the society, gives the details of the society's progress and a general account of the work it has accomplished. The volume contains, also, brief, life-like sketches of the members that have died, among whom are a number that will be long remembered in science—Dr. Benjamin D. Greene, Amos Binney, Dr. Burnett, Dr. Warren, Dr. Harris, Dr. Gould, Charles Pickering, Agassiz,

Wyman. Dr. Wyman was president for fourteen years (from 1856 to 1870), and, like Agassiz, was a man to be ever kept in mind for his excellencies by future generations of laborers in science. The Boston Society of Natural History owes much to the author of this volume for the faithful and judicious manner in which it has been prepared.

2. *American Association at Cincinnati.*—The next meeting of the American Association for the Advancement of Science opens at Cincinnati on the 17th of August. Professor GEORGE J. BRUSH, of New Haven, Conn., is President of the meeting; Professor A. M. MAYER of Hoboken, N. J., Vice-President of Section A.; F. W. PUTNAM, of Cambridge, Mass., Permanent Secretary, and C. V. RILEY, of Washington, D. C., General Secretary. The Chairman of the Subsection of Chemistry is W. R. NICHOLS, of Boston, Mass.; of Microscopy, A. B. HERVEY, of Taunton, Mass.; of Anthropology, GARRICK MALLERY, of Washington, D. C.; of Entomology, J. G. MORRIS, of Baltimore, Md.—The headquarters of the Association in the city will be at Music Hall; there will be found the offices of the Permanent Secretary and Local Committee, as well as the rooms for the sessions, and the book for registering the names of members on their arrival.

3. *On the so-called Cosmical Dust.*—Dr. LASAULX has investigated the subject of the mineral dust which at different times has been collected at various points on the earth's surface and for which a cosmical origin has been assumed. The memoir by Nordenskiöld on this subject, noticed in this Journal, ix, 145, 1875, is reviewed and some of the conclusions there reached questioned. A portion of the original material from the interior of Greenland, named by Nordenskiöld *cryoconite*, was examined microscopically and was found to be not even approximately homogeneous. On the contrary, the dust was made up of particles of quartz, mica, orthoclase and triclinic feldspars, magnetite, garnet, epidote and hornblende, and, with these, brown or brownish-green particles of organic nature probably microscopic algæ. Dr. Lasaulx concludes, from the absence of augite and chrysolite, that the dust could not have come from a volcano, but that it was derived from the gneissoid rocks on the coast of Greenland; that there is no reason to think of a cosmical origin for it, and the presence of quartz and mica declare against this idea. The dust of Catania, Sicily, which was described by Silvestri and has been regarded as cosmical, Lasaulx has also investigated. His conclusion is that all the materials present in it could have been, and in all probability were, derived from Mt. Etna. A study of the residue obtained by the melting of a large quantity of snow collected by the author in the neighborhood of Kiel revealed no minerals for which anything but a terrestrial source need be predicated. In conclusion, Lasaulx decides that the atmospheric dust is in general to be regarded as terrestrial detritus, and that before a non-terrestrial origin can be considered proved in any case, a much more critical microscopic examination must be made than has been customary in the past.

THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. XV.—*Upon a modification of Wheatstone's Microphone and its applicability to Radiophonic Researches*; by ALEXANDER GRAHAM BELL.

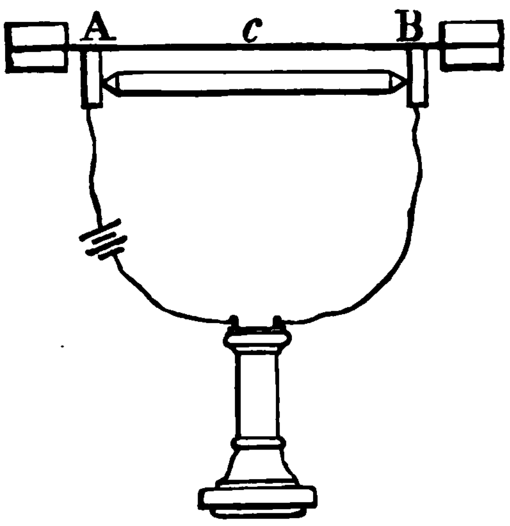
[A paper read before the Philosophical Society of Washington, D. C., June 11, 1881.]

IN August, 1880, I directed attention to the fact that thin disks or diaphragms of various materials become sonorous when exposed to the action of an intermittent beam of sunlight, and I stated my belief that the sounds were due to molecular disturbances produced in the substance composing the diaphragm.* Shortly afterwards Lord Raleigh undertook a mathematical investigation of the subject, and came to the conclusion that the audible effects were caused by the bending of the plates under unequal heating.† This explanation has recently been called in question by Mr. Preece,‡ who has expressed the opinion that although vibrations may be produced in the disks by the action of the intermittent beam, such vibrations are not the cause of the sonorous effects observed. According to him, the aerial disturbances that produce the sound arise spontaneously in the air itself by sudden expansion due to heat communicated from the diaphragm—every increase of heat giving rise to a fresh pulse of air. Mr. Preece was led to discard the theoretical explanation of Lord Raleigh on account of the failure of experiments undertaken to test the theory.

* American Association for the Advancement of Science, Aug. 27, 1880.
† Nature, vol. xxiii, p. 274. ‡ Roy. Soc., March 10, 1881.

He was thus forced—by the supposed insufficiency of the explanation—to seek in some other direction the cause of the phenomenon observed, and, as a consequence, he adopted the ingenious hypothesis alluded to above. But the experiments which had proved unsuccessful in the hands of Mr. Preece were perfectly successful when repeated in America under better conditions of experiment, and the supposed necessity for another hypothesis at once vanished. I have shown, in a recent paper read before the National Academy of Science,* that audible sounds result from the expansion and contraction of the material exposed to the beam; and that a real to-and-fro vibration of the diaphragm occurs capable of producing sonorous effects. It has occurred to me that Mr. Preece's failure to detect with a delicate microphone the sonorous vibrations that were so easily observed in our experiments might be explained upon the supposition that he had employed the ordinary form of Hughes's microphone shown in fig. 1, and that the vibrating area was confined to the central portion of the disk. Under such circumstances it might easily happen that both the supports (A, B,) of the microphone might touch portions of the diaphragm which were practically at rest. It would of course be interesting to ascertain whether any such localization of the vibration as that supposed really occurred, and I have great pleasure in showing to you to-night the apparatus by means of which this point has been investigated. (See fig. 2.)

1.



A, B, carbon supports; *c*, diaphragm.

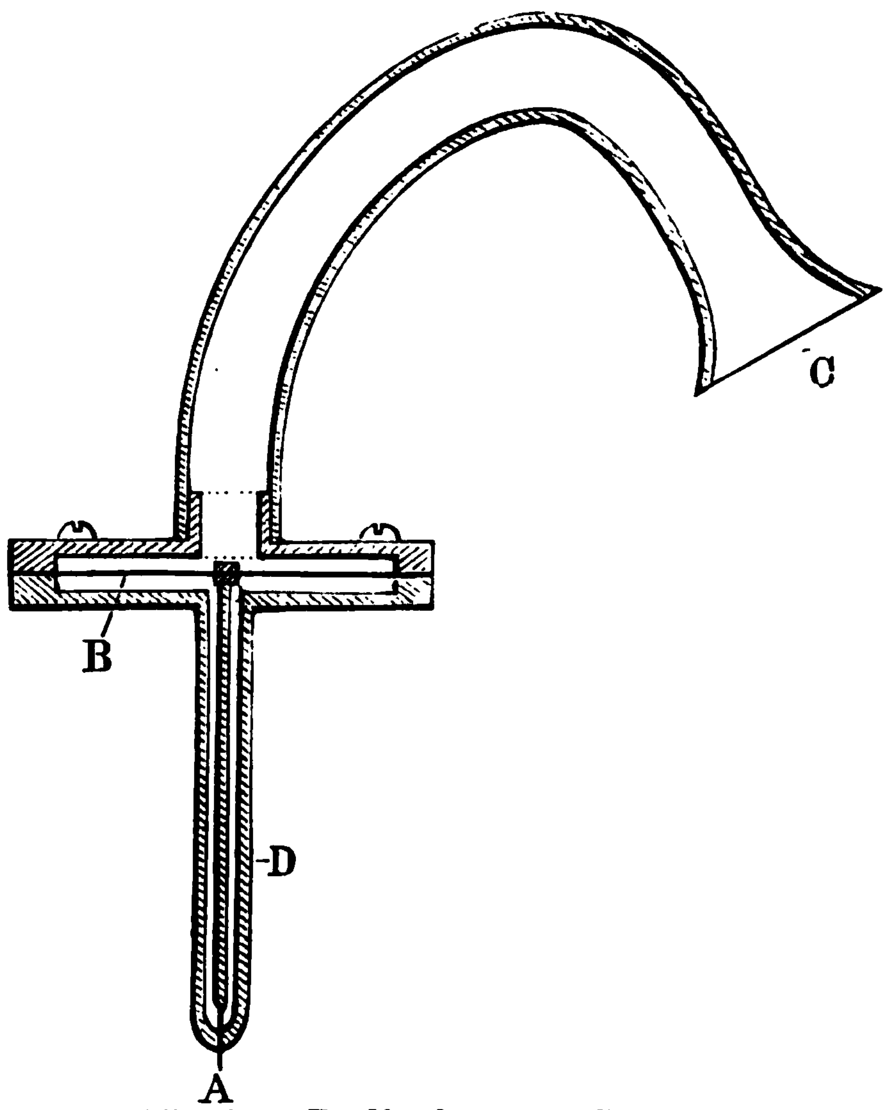
The instrument is a modification of the form of microphone devised in 1827 by the late Sir Charles Wheatstone, and it consists essentially of a stiff wire (A), one end of which is rigidly attached to the center of a metallic diaphragm (B). In Wheatstone's original arrangement the diaphragm was placed directly against the ear, and the free extremity of the wire was rested against some sounding body—like a watch. In the present arrangement the diaphragm is clamped at the circumference like a telephone-diaphragm, and the sounds are conveyed to the ear through a rubber hearing tube (*c*). The wire passes through the perforated handle (D) and is exposed only at the extremity. When the point (A) was rested against the center of a diaphragm upon which was focussed an intermittent beam of sunlight, a clear musical tone was perceived by applying the ear to the hearing tube (C). The surface of the diaphragm was

* April 21, 1881.

then explored with the point of the microphone, and sounds were obtained in all parts of the illuminated area and in the corresponding area on the other side of the diaphragm. Outside of this area on both sides of the diaphragm the sounds became weaker and weaker, until at a certain distance from the center they could no longer be perceived.

At the points where one would naturally place the supports of a Hughes microphone (see fig. 1) no sound was observed. We were also unable to detect any audible effects when the point of the microphone was rested against the support to which the diaphragm was attached. The negative results obtained in Europe by Mr. Preece may therefore be reconciled with the positive results obtained in America by Mr. Tainter and myself. A still more curious demonstration of localization of vibration occurred in the case of a large metallic mass. An intermittent beam of sunlight was focussed upon a brass weight (1 kilogram), and the surface of the weight was then explored with the microphone shown in fig. 2. A feeble but distinct sound was heard upon touching the surface within the illuminated area and for a short distance outside, but not in other parts.

2.



A, stiff wire; B, diaphragm; C, hearing tube; D, perforated handle. Figure reduced one-half.

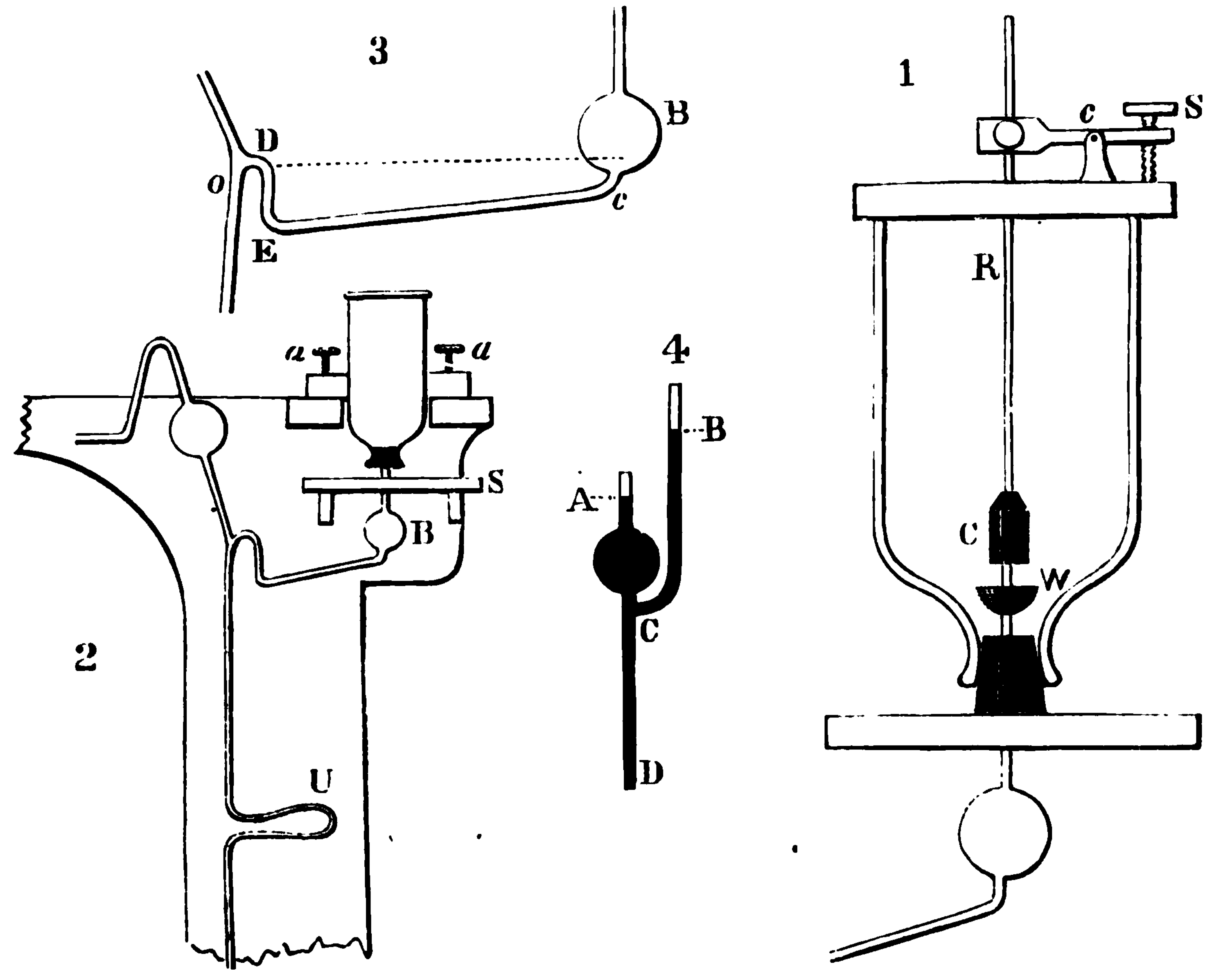
In this experiment, as in the case of the thin diaphragm, absolute contact between the point of the microphone and the surface explored was necessary in order to obtain audible effects. Now I do not mean to deny that sound waves may be originated in the manner suggested by Mr. Preece, but I think that our experiments have demonstrated that the kind of action described by Lord Raleigh actually occurs, and that it is sufficient to account for the audible effects observed.

ART. XVI.—*On a method of obtaining and measuring very high Vacua with a modified form of Sprengel-pump;* by OGDEN N. ROOD, Professor of Physics in Columbia College.

IN the July number of this Journal for 1880, I gave a short account of certain changes in the Sprengel-pump by means of which far better vacuua could be obtained than had been previously possible. For example, the highest vacuum at that time known had been reached by Mr. Crooks, and was about $\frac{1}{17\,000\,000}$, while with my arrangement vacuua of $\frac{1}{100\,000\,000}$ were easily reached. In a notice that appeared in "Nature" for August, 1880, p. 375, it was stated that my improvements were not new, but had already been made in England four years previously. I have been unable to obtain a printed account of the English improvements, and am willing to assume that they are identical with my own; but, on the other hand, as for four years no particular result seems to have followed their introduction in England, I am reluctantly forced to the conclusion that their inventor and his customers, for that period of time, have remained quite in ignorance of the proper mode of utilizing them. Since then I have pushed the matter still farther, and have succeeded in obtaining with my apparatus vacuua as high as $\frac{1}{390\,000\,000}$, without finding that the limit of its action had been reached. The pump is simple in construction, inexpensive and, as I have proved by a large number of experiments, certain in action and easy of use: stopcocks and grease are dispensed with, and when the presence of a stopcock is really desirable its place is supplied by a movable column of mercury.

Reservoir.—An ordinary inverted bell-glass with a diameter of 100^{mm} and a total height of 205^{mm} forms the reservoir; its mouth is closed by a well-fitting cork through which passes the glass tube that forms one termination of the pump. The cork around tube and up to the edge of the former is painted with a flexible cement. The tube projects 40^{mm} into the mercury and passes through a little watch-glass-shaped piece of sheet-iron, W, figure 1, which prevents the small air bubbles that creep upward along the tube from reaching its open end; the little cup is firmly cemented in its place. The flow of the mercury is regulated by the steel rod and cylinder CR, figure 1. The bottom of the steel cylinder is filled out with a circular piece of pure india-rubber, properly cemented; this soon fits itself to the use required and answers admirably. The pressure of the cylinder on the end of the tube is regulated by the lever S, figure 1; this is attached to a circular board which

again is firmly fastened over the open end of the bell-glass. It will be noticed that on turning the milled head S, the motion of the steel cylinder is not directly vertical, but that it tends to describe a circle with *c* as a center; the necessary play of the cylinder is however so small, that practically the experimenter does not become aware of this theoretical defect, so that the arrangement really gives entire satisfaction, and after it has been in use for a few days accurately controls the flow of the mercury. The glass cylinder is held in position, but not supported, by two wooden *adjustable* clamps *a a*, figure 2. The weight of the cylinder and mercury is supported by a shelf, S, figure 2, on which rests the cork of the cylinder; in this way all danger of a very disagreeable accident is avoided.



Vacuum-bulb.—Leaving the reservoir, the mercury enters the vacuum-bulb B, figure 2, where it parts with most of its air and moisture; this bulb also serves to catch the air that creeps into the pump from the reservoir, even when there is no flow of mercury; its diameter is 27^{mm}. The shape and inclination of the tube attached to this bulb is by no means a matter of indifference; accordingly figure 3 is a separate drawing of it; the tube should be so bent that a horizontal line drawn from the proper level of the mercury in the bulb passes through the point *o*, where the drops of mercury break off. The length of the tube EC should be 150^{mm}, that of the tube ED 45^{mm}; the bore of this tube is about the same as that of the fall-tube.

Fall-tube and bends.—The bore of the fall-tube in the pump now used by me is 1·78^{mm}; its length above the bends (U, figure 2) is 310^{mm}; below the bends the length is 815^{mm}. The bends constitute a fluid valve that prevents the air from returning into the pump; beside this, the play of the mercury in them greatly facilitates the passage of the air downward. The top of the mercury column representing the existing barometric pressure should be about 25^{mm} below the bends when the pump is in action. This is easily regulated by an adjustable shelf, which is also employed to fill the bends with mercury when a measurement is taken or when the pump is at rest. On the shelf is a tube, 160^{mm} high and 20^{mm} in diameter, into which the end of the fall-tube dips; its side has a circular perforation into which fits a small cork with a little tube bent at right angles. With the hard end of a file and a few drops of turpentine the perforation can be easily made and shaped in a few minutes. By revolving the little bent tube through 180° the flow of the mercury can be temporarily suspended when it is desirable to change the vessel that catches it.

Gauge.—For the purpose of measuring the vacua I have used an arrangement similar to McLeod's gauge, fig. 4; it has, however, some peculiarities. The tube destined to contain the compressed air has a diameter of 1·35^{mm}, as ascertained by a compound microscope; it is not fused at its upper extremity, but closed by a fine glass rod that fits into it as accurately as may be, the end of the rod being ground flat and true. This rod is introduced into the tube, and while the latter is gently heated a very small portion of the cement described below is allowed to enter by capillary attraction, but not to extend beyond the end of the rod, the operation being watched by a lens. The rod is used for the purpose of obtaining the compressed air in the form of a cylinder and also to allow cleansing of the tube when necessary. The capacity of the gauge-sphere was obtained by filling it with mercury; its external diameter was sixty millimeters; for measuring very high vacua this is somewhat small and makes the probable errors rather large; I would advise the use of a gauge-sphere of about twice as great capacity. The tube CB, figure 4, has the same bore as the measuring tube in order to avoid corrections for capillarity. The tube of the gauge CD is not connected with an india-rubber tube, as is usual, but dips into mercury contained in a cylinder 340^{mm} high, 58^{mm} in diameter, which can be raised and lowered at pleasure. This is best accomplished by the use of a set of boxes of various thicknesses, made for the purpose and supplemented by several sheets of cardboard and even of writing-paper. These have been found to answer well and enable the experimenter to graduate with a nicety the pressure

to which the gas is exposed during measurement. By employing a cylinder filled with mercury instead of the usual caoutchouc tubing small bubbles of air are prevented from entering the gauge along with the mercury. An adjustable brace or support is used which prevents accident to the cylinder when the pump is inclined for the purpose of pumping out the vacuum-bulb. The maximum pressure that can be employed in the gauge used by me is 100^{mm}.

All the tubing of the pump is supported at a distance of about 55^{mm} from the wood-work; this is effected by the use of simple adjustable supports and adjustable clamps; the latter have proved a great convenience. The object is to gain the ability to heat with a Bunsen burner all parts of the pump without burning the wood-work. Where glass and wood necessarily come in contact the wood is protected by metal or simply painted with a saturated solution of alum. The glass portions of the pump I have contrived to anneal completely by the simple means mentioned below. If the glass is not annealed it is certain to crack when subjected to heat, thus causing vexation and loss of time. The mercury was purified by the same method that was used by W. Siemens (Pogg. Annalen, vol. cx, p. 20), that is, by a little strong sulphuric acid to which a few drops of nitric acid had been added; it was dried by pouring it repeatedly from one hot dry vessel to another, by filtering it while quite warm, the drying being completed finally by the action of the pump itself. All the measurements were made by a fine cathetometer which was constructed for me by William Grunow; see this Journal, Jan., 1874, p. 23. It was provided with a well-corrected object-glass having a focal length of 200^{mm}, and as used by me gave a magnifying power of 16 diameters.

Manipulation.—The necessary connections are effected with a cement made by melting Burgundy pitch with three or four per cent of gutta percha. It is indispensable that the cement when cold should be so hard as completely to resist taking any impression from the finger nail, otherwise it is certain to yield gradually and finally to give rise to leaks. The connecting tubes are selected so as to fit as closely as possible, and after being put into position are heated to the proper amount, when the edges are touched with a fragment of cold cement which enters by capillary attraction and forms a transparent joint that can from time to time be examined with a lens for the colors of thin plates, which always precede a leak. Joints of this kind have been in use by me for two months at a time without showing a trace of leakage, and the evidence gathered in another series of unfinished experiments goes to show that no appreciable amount of vapor is furnished by the

resinous compound, which, I may add, is never used until it has been repeatedly melted. As drying material I prefer caustic potash that has been in fusion just before its introduction into the drying tube; during the process of exhaustion it can from time to time be heated nearly to the melting point; if actually fused in the drying tube the latter almost invariably cracks. The pump in the first instance is to be inclined at an angle of about 10 degrees, the tube of the gauge being supported by a semicircular piece of thick paste-board fitted with two corks into the top of the cylinder. This seemingly awkward proceeding has in no case been attended with the slightest accident, and owing to the presence of the four leveling-screws the pump when righted returns, as shown by the telescope of the cathetometer, almost exactly to its original place. In the inclined position the exhaustion of the vacuum-bulb is accomplished along with that of the rest of the pump. The exhaustion of the vacuum-bulb when once effected can be preserved to a great extent for use in future work, merely by allowing mercury from the reservoir to flow in a rapid stream at the time that air is allowed to reënter the pump. During the first process of exhaustion the tube of the gauge is kept hot by moving to and fro a Bunsen burner, and is in this way freed from those portions of air and moisture that are not too firmly attached. After a time the vacuum bulb ceases to deliver bubbles of air; it and the attached tube are now to be heated with a moving Bunsen-burner, when it will be found to furnish for 15 or 20 minutes a large quantity of bubbles mainly of vapor of water. After their production ceases the pump is righted and the exhaustion carried farther. In spite of a couple of careful experiments with the cathetometer I have not succeeded in measuring the vacuum in the vacuum-bulb, but judge from indications, that is about as high as that obtained in an ordinary Geissler pump. Meanwhile the various parts of the pump can be heated with a moving Bunsen-burner to detach air and moisture, the cement being protected by wet lamp-wicking. In one experiment I measured the amount of air that was detached from the walls of the pump by heating them for 10 minutes somewhat above 100° C., and found that it was $\frac{1}{1\,000\,000}$ of the air originally present. I have also noticed that a still larger amount of air is detached by electric discharges. This coincides with an observation of E. Bessel-Hagen in his interesting article on a new form of Töpler's mercury-pump (Annalen der Physik und Chemie, 1881, vol. xii). Even when potash is used a small amount of moisture always collects in the bends of the fall-tube; this is readily removed by a Bunsen-burner; the tension of the vapor being greatly increased, it passes far down the fall-tube in large

bubbles and is condensed. Without this precaution I have found it impossible to obtain a vacuum higher than $\frac{1}{25\,000\,000}$; in point of fact the bends should always be heated when a high exhaustion is undertaken even if the pump has been standing well exhausted for a week; the heat should of course never be applied at a late stage of the exhaustion. Conversely, I have often by the aid of heat completely and quickly removed quite large quantities of the vapor of water that had been purposely introduced. The exhaustion of the vacuum-bulb is of course somewhat injured by the act of using the pump and also by standing for several days, so that it has been usual with me before undertaking a high exhaustion to incline the pump and reëxhaust for 20 minutes; I have however obtained very high vacua without using this precaution.

During the process of exhaustion not more than one-half of the mercury in the reservoir is allowed to run out, otherwise when it is returned bubbles of air are apt to find their way into the vacuum-bulb. In order to secure its quiet entrance it is poured into a silk bag provided with several holes. When the reservoir is first filled its walls for a day or two appear to furnish air that enters the vacuum-bulb; this action, however, soon sinks to a minimum and then the leakage remains quite constant for months together.

Measurement of the vacuum.—The cylinder into which the gauge-tube dips is first elevated by a box sufficiently thick merely to close the gauge, afterwards boxes are placed under it sufficient to elevate the mercury to the base of the measuring tube; when the mercury has reached this point, thin boards and card-boards are added till a suitable pressure is obtained. The length of the enclosed cylinder of air is then measured with the cathetometer, also the height of the mercurial "meniscus," and the difference of the heights of the mercurial columns in A and B, figure 4. To obtain a second measure an assistant removes some of the boxes and the cylinder is lowered by hand three or four centimeters and then replaced in its original position. In measuring really high vacua, it is well to begin with this process of lowering and raising the cylinder, and to repeat it five or six times before taking readings. It seems as though the mercury in the tube B supplies to the glass a coating of air that allows it to move more freely; at all events it is certain that ordinarily the readings of B become regular, only after the mercury has been allowed to play up and down the tube a number of times. This applies particularly to vacua as high as $\frac{1}{50\,000\,000}$ and to pressures of five millimeters and under. It is advantageous in making measurements to employ large pressures and small volumes; the correct working of the gauge can from time to time be tested by varying the relations

of these to each other. This I did quite elaborately, and proved that such constant errors as exist, are small, compared with inevitable accidental errors, as for example that there was no measurable correction for capillarity, that the calculated volume of the "meniscus" was correct, etc. It is essential in making a measurement that the temperature of the room should change as little as possible, and that the temperature of the mercury in the cylinder should be at least nearly that of the air near the gauge-sphere. The computation is made as follows:

n=height of the cylinder enclosing the air;
c=a factor which multiplied by n converts it into cubic millimeters;
S=cubic contents of the meniscus;
d=difference of level between A and B, fig. 4;
=the pressure the air is under;
N=the cubic contents of the guage in millimeters;
x=a fraction expressing the degree of exhaustion obtained: then

$$x = \frac{1}{\dfrac{N\dfrac{760}{d}}{nc - S}}$$

It will be noticed that the measurements are independent of the actual height of the barometer, and if several readings are taken continuously, the result will not be sensibly affected by a simultaneous change of the barometer. Almost all the readings were taken at a temperature of about 20° C., and in the present state of the work corrections for temperature may be considered a superfluous refinement.

Gauge correction.—It is necessary to apply to the results thus obtained a correction which becomes very important when high vacua are measured. It was found in an early stage of the experiments that the mercury in the act of entering the highly exhausted gauge, gave out invariably a certain amount of air which of course was measured along with the residuum that properly belonged there; hence to obtain the true vacuum it is necessary to subtract the volume of this air from nc. By a series of experiments I ascertained that the amount of air introduced by the mercury in the acts of entering and leaving the gauge was sensibly constant for six of these single operations (or for three of these double operations), when they followed each other immediately. The correction accordingly is made as follows: the vacuum is first measured as described above, then by withdrawing all the boxes except the lowest, the mercury is allowed to fall so as nearly to empty the gauge; it is then made again to fill the gauge, and these operations are repeated until they amount in all to six; finally the volume and pressure

are a second time measured. Assuming the pressure to remain constant, or that the volumes are reduced to the same pressure,

v=the original volume;
v'=the final volume;
V'=volume of air introduced by the first entry of the mercury;
V=corrected volume; then

$$V' = \frac{v' - v}{6}$$

$$V = v - \frac{v' - v}{6}$$

It will be noticed that it is assumed in this formula that the same amount of air is introduced into the gauge in the acts of entry and exit; in the act of entering in point of fact more fresh mercury is exposed to the action of the vacuum than in the act exit, which might possibly make the true gauge-correction rather larger than that given by the formula. It has been found that when the pump is in constant use the gauge-correction gradually diminishes from day to day: in other words, the air is gradually pumped out of the gauge-mercury. Thus on December 21st, the amount of air entering with the mercury corresponded to an exhaustion of

$\frac{1}{27\,308\,805}$ ......Dec. 21st.

$\frac{1}{38\,806\,688}$ ......Dec. 29th.

$\frac{1}{78\,125\,000}$ ......Jan. 15th.

$\frac{1}{83\,333\,333}$ ......Jan. 23d.

$\frac{1}{128\,834\,063}$ ......Feb. 1st.

$\frac{1}{226\,757\,400}$ ......Feb. 9th.

$\frac{1}{232\,828\,800}$ ......Feb. 19th.

$\frac{1}{388\,200\,000}$ ......March 7th.

That this diminution is not due to the air being gradually withdrawn from the walls of the gauge or from the gauge-tube, is shown by the fact that during its progress the pump was several times taken to pieces, and the portions in question exposed to the atmosphere without affecting the nature or extent of the change that was going on. I also made one experiment which proves that the gauge-correction does not

increase sensibly, when the exhausted pump and gauge are allowed to stand unused for twenty days.

Rate of the pump's work.—It is quite important to know the rate of the pump at different degrees of exhaustion, for the purpose of enabling the experimenter to produce a definite exhaustion with facility: also if its maximum rate is known and the minimum rate of leakage, it becomes possible to calculate the highest vacuum attainable with the instrument. Examples are given in the tables below: the total capacity was about 100,000 cubic mm.

Time.	Exhaustion.	Ratio.
	$\frac{1}{78\ 511}$	
10 minutes		$1 : \frac{1}{3{\cdot}53}$
	$\frac{1}{276\ 980}$	
10 minutes		$1 : \frac{1}{6{\cdot}10}$
	$\frac{1}{1\ 687\ 140}$	
10 minutes		$1 : \frac{1}{4{\cdot}15}$
	$\frac{1}{7\ 002\ 000}$	

Upon another occasion the following rates and exhaustions were obtained:

Time.	Exhaustion.	Rate.
	$\frac{1}{7\ 812\ 500}$	
10 minutes		$1 : \frac{1}{3{\cdot}18}$
	$\frac{1}{24\ 875\ 620}$	
10 minutes		$1 : \frac{1}{2{\cdot}69}$
	$\frac{1}{67\ 024\ 090}$	
10 minutes		$1 : \frac{1}{1{\cdot}22}$
	$\frac{1}{81\ 760\ 810}$	
10 minutes		$1 : \frac{1}{1{\cdot}67}$
	$\frac{1}{136\ 986\ 300}$	
10 minutes		$1 : \frac{1}{1{\cdot}23}$
	$\frac{1}{170\ 648\ 500}$	

The *irregular* variations in the rates are due to the mode in which the flow of the mercury was in each case regulated.

Leakage.—We come now to one of the most important elements in the production of high vacua. After the air is detached from the walls of the pump the leakage becomes and remains nearly constant. I give below a table of leakages, the pump being in each case in a condition suitable for the production of a very high vacuum :

Duration of the experiment.	Leakage per hour in cubic mm., press. 760^{mm}.
18½ hours	·000853
27 hours	·001565
26½ hours	·000791
20 hours	·000842
19 hours	·000951
19 hours	·001857
7 days	·001700
7 days	·001574
Average	·001266

I endeavored to locate this leakage, and proved that one-quarter of it is due to air that enters the gauge from the top of its column of mercury, thus:

Duration of the experiment.	Gauge-leakage per hour in cubic mm., press. 760^{mm}.
18 hours	·0002299
7 days	·0004093
7 days	·0003464
Average	·0003285

This renders it very probable that the remaining three-quarters are due to air given off from the mercury at B, fig. 4, from that in the bends and at the entrance of the fall-tube *o*, fig. 3. Farther on some evidence will be given that renders it probable that the leakage of the pump when in action is about four times as great as the total leakage in a state of rest.

The gauge, when arranged for measurement of gauge-leakage, really constitutes a barometer, and a calculation shows that the leakage would amount to 2·877 cubic millimeters per year press. 760^{mm}. If this air were contained in a cylinder 90^{mm} long and 15^{mm} in diameter it would exert a pressure of ·14^{mm}. To this I may add that in one experiment I allowed the gauge for seven days to remain completely filled with mercury and then measured the leakage into it. This was such as would in a year amount to ·488 cubic millimeters press. 760^{mm}, and in a cylinder of the above dimensions would exert a pressure of ·0233^{mm}.

Reliability of results; highest vacuum.

The following are samples of the results obtained. In one case sixteen readings were taken in groups of four with the following result:

Exhaustion.

$$\frac{1}{74\,219\,139}$$

$$\frac{1}{78\,533\,454}$$

$$\frac{1}{79\,017\,272}$$

$$\frac{1}{68\,503\,182}$$

$$\text{Mean}\quad \frac{1}{74\,853\,449}$$

Calculating the probable error of the mean with reference to the above four results it is found to be 2·28 per cent of the quantity involved.

A higher vacuum measured in the same way gave the following results:

$$\frac{1}{146\,198\,800}$$

$$\frac{1}{175\,131\,300}$$

$$\frac{1}{204\,081\,600}$$

$$\frac{1}{201\,207\,200}$$

The mean is $\frac{1}{178\,411\,934}$, with a probable error of 5·42 per cent of the quantity involved. I give now an extreme case; only five single readings were taken; these corresponded to the following exhaustions:

$$\frac{1}{379\,219\,500}$$

$$\frac{1}{371\,057\,265}$$

$$\frac{1}{250\,941\,040}$$

$$\frac{1}{424\,088\,232}$$

$$\frac{1}{691\,082\,540}$$

The mean value is $\frac{1}{381\,100\,000}$, with a probable error of 10·36 per cent of the quantity involved. Upon other occasions I have obtained exhaustions of $\frac{1}{373\,134\,000}$ and $\frac{1}{388\,200\,000}$. Of course in these cases a gauge-correction was applied; the highest vacuum that I have ever obtained irrespective of a gauge-correction was $\frac{1}{190\,392\,150}$. In these cases and in general, potash was employed as the drying material; I have found it practical, however, to attain vacua as high as $\frac{1}{50\,000\,000}$ in the total absence of all such substances. The vapor of water which collects in bends must be removed from time to time with a Bunsen-burner while the pump is in action.

It is evident that the final condition of the pump is reached when as much air leaks in per unit of time as can be removed in the same interval. The total average leakage per ten minutes in the pump used by me, when at rest, was ·000211 cubic millimeters at press. 760^{mm}. Let us assume that the leakage when the pump is in action is four times as great as when at rest; then in each ten minutes ·000844 cubic millimeters press. 760^{mm} would enter; this corresponds in the pump used by me to an exhaustion of $\frac{1}{124\,000\,000}$; if the rate of the pump is such as to remove one-half of the air present in ten minutes, then the highest attainable exhaustion would be $\frac{1}{248\,000\,000}$. In the same way it may be shown that if six minutes are required for the removal of half the air the highest vacuum would be $\frac{1}{413\,000\,000}$ nearly, and rates even higher than this have been observed in my experiments. An arrangement of the vacuum-bulb whereby the entering drops of mercury would be exposed to the vacuum in an isolated condition for a somewhat longer time would doubtless enable the experimenter to obtain considerably higher vacua than those above given.

Exhaustions obtained with a plain Sprengel-pump.—I made a series of experiments with a plain Sprengel-pump without stopcocks, and arranged, as far as possible, like the instrument just described. The leakage per hour was as follows:

Duration of the experiment.	Leakage per hour in cubic mm. at press. 760^{mm}.
22 hours	·04563
2 days	·04520
2 days	·09210
4 days	·06428
Mean	·0618

Using the same reasoning as above we obtain the following table:

Time necessary for removal of ½ the air.	Greatest attainable exhaustion.
10 minutes	$\frac{1}{5\,000\,000}$
7·5 minutes	$\frac{1}{7\,000\,000}$
6·6 minutes	$\frac{1}{12\,000\,000}$

In point of fact the highest exhaustion I ever obtained with this pump was $\frac{1}{5\,000\,000}$; from which I infer that the leakage during action is considerably greater than four times that of the pump at rest. The general run of the experiments tends to show that the leakage of a plain Sprengel-pump, without stopcocks or grease, is, when in action, about 80 times as great as in the form used by me.

Note on annealing glass tubes.—It is quite necessary to anneal all those parts of the pump that are to be exposed to heat, otherwise they soon crack. I found by enclosing the glass in heavy iron tubes and exposing it for five hours to a temperature somewhat above that of melting zinc, and then allowing an hour or two for the cooling process, that the strong polarization figure which it displays in a polariscope was completely removed, and hence the glass annealed. A common gas-combustion furnace was used, the bends, etc., being suitably enclosed in heavy metal and heated over a common ten-fold Bunsen-burner. Thus far no accident has happened to the annealed glass, even when cold drops of mercury struck in rapid succession on portions heated considerably above 100° C.

I wish, in conclusion, to express my thanks to my assistant, Dr. Ihlseng, for the labor he has expended in making the large number of computations necessarily involved in work of this kind.

New York, June 10, 1881

ART. XIX. — *Geological Relations of the Limestone Belts of Westchester County, New York;* by JAMES D. DANA.

ORIGIN OF THE ROCKS OF THE "CORTLANDT SERIES."

IN the account of the massive Cortlandt rocks* I have shown that, although Archæan-like in the presence of the hypersthene-rock, noryte, in the abundance of hornblende and augite, and the occurrence of corundum-bearing magnetite beds, a large part of them afford evidence of conformability to the associated schists and limestone strata of the country, as if one with them in metamorphic origin; and that if any were truly eruptive these were in part more recent than the limestone, since they cut through it at Verplanck Point. They hence present nothing against the chronological conclusion which has been reached.

These rocks, however, are so limited in distribution, and so peculiar in composition—being often chrysolitic, always having soda-lime feldspar predominant, and containing little or no quartz—that it becomes an interesting question, Whence their abrupt interpolation among the schists and limestones of the region.

That the lithological facts may be in mind preparatory to the following discussion I here re-mention the prominent kinds of rocks.

1. *Soda-granite:* granite-like, consisting chiefly of oligoclase and biotite, with little quartz, and often containing some hornblende; varying from coarse to fine in grain, and very light-colored to black—the black very micaceous and fine-grained.

2. *Dioryte, Quartz-dioryte:* chiefly oligoclase and hornblende, with more or less biotite, and a little quartz; varying from very coarse and granite-like to fine-grained.

3. *Noryte:* chiefly the feldspar, andesite—or, more probably, its equivalent, 1 of labradorite and 2 of obligoclase—and hypersthene, with more or less augite and biotite; usually dark gray or reddish brown in color, and rather finely granular; the hypersthene often in small crystals seldom exceeding a sixth of an inch in length, and never in folia.

4. *Augite-noryte:* like the noryte in aspect and constitution, but containing augite in place of the hypersthene.

5. *Hornblendyte:* coarsely crystalline; chiefly black hornblende in small or large cleavable individuals.

6. *Pyroxenyte:* rather coarsely crystalline; chiefly augite, but sometimes a grayish-green pyroxene.

7 to 9. *Chrysolitic hornblendyte, chrysolitic pyroxenyte,* with some chrysolitic noryte.

* This Journal, for September last, III, xx, 194.

Other constituents of these rocks are frequently apatite (which is often in unusual proportions), and more or less magnetite, pyrrhotite and pyrite (the pyrite mostly confined to the soda-granite and dioryte). In the many slices (over 60) which I have microscopically examined, I have found no glassy or unindividalized material, and no appearances of a fluidal character, except that of broken crystals or crystalline grains.

To the description of the noryte before given I here add the results of a careful chemical analysis made in the laboratory of the Sheffield Scientific School of Yale College (under Professor O. D. Allen) by Mr. M. D. Munn of that School. The specimen was from the northern half of Montrose Point, on the Hudson.

	SiO_2	AlO_3	FeO_3	FeO	MnO	MgO	CaO	Na_2O	K_2O	H_2O	
1.	55·28	16·31	0·69	7·57	0·40	5·05	7·52	4·10	2·05	0 58	=99·55
2.	55·40	16·44	0·85	7·51	0·39	5·05	7·49	4·03	2·00	[0·58]	=99·73
Mean	55·34	16·37	0·77	7·54	0·40	5·05	7·51	4·06	2·03	0·58	=99·65

A trace of CO_2 also was obtained.

To the eye it appeared to contain about as much hypersthene as augite, the crystals of the former being distinguished by a brighter and somewhat bronze-like luster on a cleavage surface, and a less black color; and this proportion was confirmed, as far as could be done, by a microscopic examination of a thin slice. There was present also a little black mica, and some magnetite. The results of the analysis may correspond, if 1·50 of the potash replaces part of the soda, to about 61 per cent of andesite, 33 of bisilicates, 5 of biotite and 1 of magnetite. But part of the potash may be present in orthoclase, and the andesite be, as above recognized, a mixture of labradorite and oligoclase. The analysis appears to show that in constitution the rock approaches closely the dioryte of the region, but with this important difference, that hypersthene and augite are present in place of hornblende and the feldspar portion is more largely basic. The relation to the noryte is much nearer, for one rock graduates into the other; and the hypersthene, which is the characteristic mineral of the former, has the same cleavage angle as augite, and the same constituents, magnesia and iron protoxide, the augite affording besides only lime. Hence the name *augite-noryte* for the rock is appropriate. It has the mineral constitution of the so-called augite-andesyte, and also of a part of what has been included by some writers under the name melaphyre.

The evidence already presented with regard to the Cortlandt rocks sustains the conclusion, as I believe, that to a large extent at least they are of metamorphic origin; but that in the metamorphic process the original beds were rendered (through the heated moisture concerned in the metamorphism), more or less plastic or mobile, so that they thus lost all, or the most of, their original bedding, and that, as a consequence, they formed in some places intrusive dikes or veins intersecting other rocks having all the characteristics of eruptive rocks.

But if "to a large extent" metamorphic, that is, altered sedimentary beds, Why were there, in that narrow corner of Westchester County, covering but twenty-five square miles, beds so unlike ordinary sediments in consisting of the materials of soda-lime feldspars, hornblende, pyroxene, and chrysolite, when, close around and throughout the county to its eastern and southern limits, only ingredients occurred for making common mica schists and gneisses with subordinate layers of hornblende schist?

Before proceeding to this topic I will first mention the facts as to the special geographical position of the area covered by the Cortlandt rocks; and, secondly, briefly review the evidence as to their metamorphic origin. We shall then be prepared to enquire into the source or sources of the material.

1. *Geographical Position of the Area.*

The small region of Cortlandt rocks is situated in the vicinity of the Hudson, near where this river leaves its channel through the Archæan Highlands. This relation to the position of the Archæan and the river channel is shown on the following map (p. 106). Upon it, the Archæan area is the black portion dotted with small vs, crossing the Hudson, from southwest to northeast, between Moodna and Fishkill on the north and Peekskill on the south: and the Cortlandt rocks occupy the area east of the Archæan, south and southeast of Peekskill on the east of the Hudson, and on Stony Point (ST) on the west side of this river. Near Peekskill the Cortlandt area is separated from the Archæan by belts of limestone (horizontally lined on the map), quartzyte, argillyte-like hydromica schist and mica schist, in all one to three miles in width; and that of Stony Point has, between it and the Archæan, a continuation of the same rocks (the limestone area on the map being, as elsewhere, horizontally lined, and that of the slates, which are partly quartzyte, distinguished by a vertical lining with white and dotted bands). The portion of the map north of the Archæan and occupying valleys within its area, has been already explained as Lower Silurian; (1) limestone, (2) slates or schists (vertically lined), and (3) quartzyte (dotted), the limestone and schist in places fossiliferous; and as part of the great formation which comprises and is continuous with the true Taconic schists and limestone to the northeast, and the recognized Lower Silurian rocks of New Jersey and the States to the southwest.

The larger map of western Cortlandt from Peekskill to Cruger's (comprising the Verplanck peninsula) and also Stony Point is reproduced on the following page, that the positions of the several localities and of the limestone belts may be

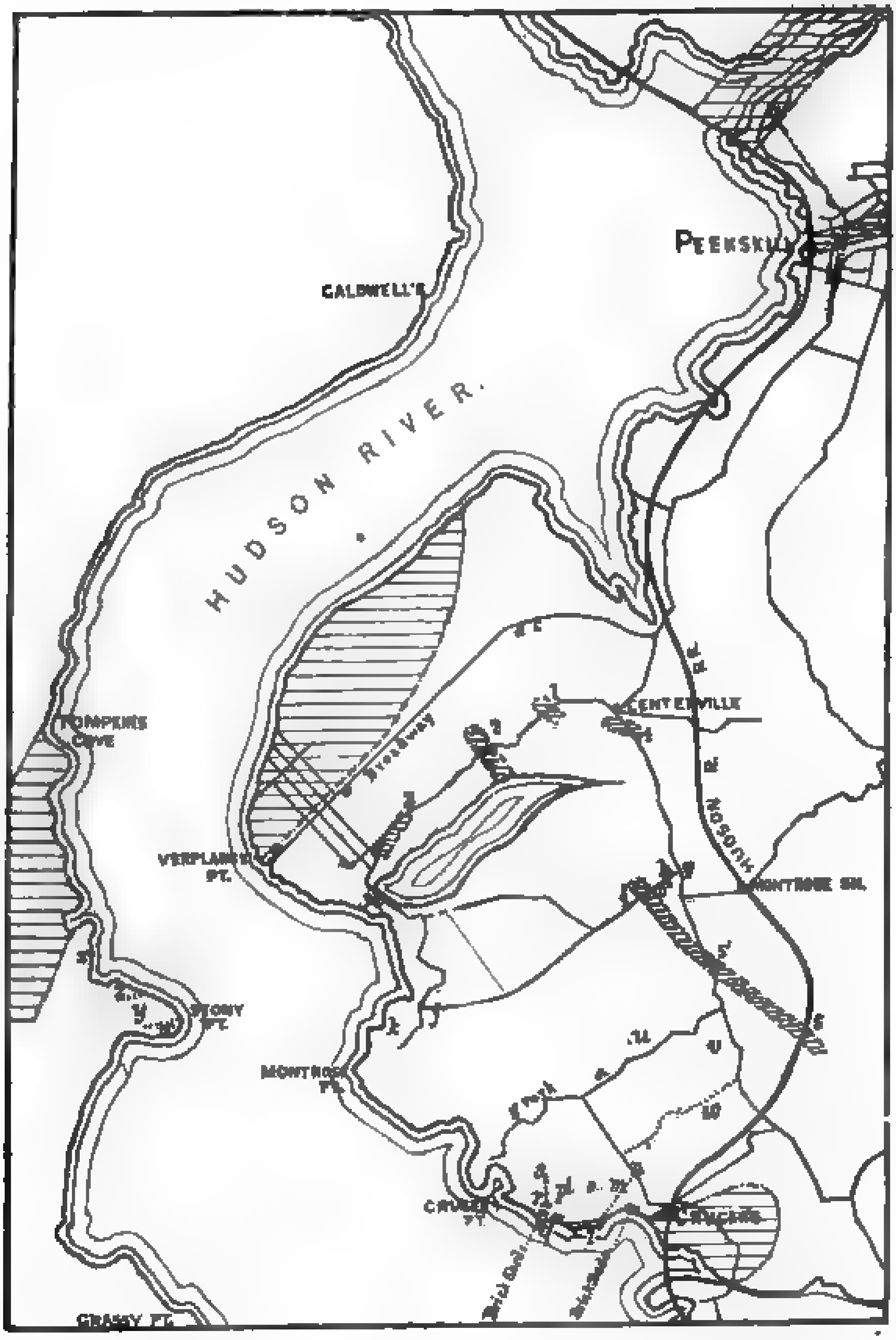
more distinctly before the reader, and especially the relations of Stony Point to Montrose Point and other places on the east side of the Hudson.

The eastern outline of the Archæan makes a large angle at the crossing of the Hudson (the course on the west being northeast, and that on the east, east-northeast), so that the form was, thus far at least, favorable for the existence there of a broad bay in the Lower Silurian sea. The river-channel through the

Map of parts of New York and New Jersey: ST, Stony Point, on the west side of the Hudson; V, Verplanck Point, on the east side; CR, limestone about Cruger's Station. Scale, 10 miles to 1 inch.

Highlands had not yet been made, as is indicated by the continuity of the Lower Silurian beds on the north of the Highland area across from Fishkill, and that of the same on the south across from Peekskill. The Lower Silurian ocean extended over the Cortlandt area, and here were spread out the sand-beds and muds that now constitute the quartzyte and slates of the Potsdam or Primordial (Cambrian) period and the material of the limestone formation. North of the Archæan, in the Fishkill, Newburgh and Poughkeepsie regions, fossils found in the limestones and hydromica schist have demonstrated that the beds there are beyond question Lower Silurian; and

the like conformable association of quartzyte, slate and semi-crystalline limestone in the Peekskill region, together with their



Map of part of Western Cortlandt, showing the Peekskill, Verplanck, Tompkins Cove, and Cruger limestone areas, by horizontal lining. Scale, 1 inch to a mile.

unconformability to the Archæan, and their relation to New Jersey limestones have been adduced, in my former paper, as proof of a like Lower Silurian age for the Peekskill beds.

A freshwater stream must have emptied into this Cortlandt bay near the present channel of the Hudson; for the general surface of the Highland area and the course of the existing streams over its surface have a pitch southward; but the length of this young Hudson River could hardly have equalled ten miles; for these old lands, as the Lower Silurian in its valleys prove, stood at a lower level than now. This little stream was the chief one that gave aid to the ocean's waters in the work of distributing Archæan detritus over the Cortlandt area. Nothing could have come down the valleys called Canopus Hollow and Peekskill Hollow; for these were for several miles arms of the sea in which limestone beds were accumulating. The cut through the Highlands now occupied by the Hudson was probably begun in a fracture during the making of the Green Mountains at the close of the Lower Silurian.

2. *Metamorphic origin of the Rocks.*

The following are the principal points in the evidence sustaining the view that the rocks are, to a large extent, metamorphosed sedimentary beds.

(1.) The mica schist or micaceous gneiss in several places graduates into the soda-granite along the plane of contact, though always rather abruptly.

(2.) The soda-granite, near its junction with the schist, and sometimes remote from it, contains, at short intervals, distinct layers of the schist, in positions conformable to the bedding outside, and single beds of this kind are in some cases continuous beds for 200 feet or more.

(3.) The mica schist at Cruger's in some parts contains beds that consist largely of staurolite, fibrolite, and magnetite (all infusible species), with abundant scales of silvery mica, a mineral that fuses with great difficulty; and the layers of schist which are in the soda-granite, just north, have a similar constitution; as if they owed their resistance to the fusion which the rest experienced because of their consisting chiefly of these refractory materials.

(4.) The noryte and chrysolite rocks contain, occasionally, similar included conformable beds of schist; and some of these are beds of magnetite and corundum, with fibrolite, that is, are beds of emery; and the noryte is sometimes crossed by gneissic layers and has occasional planes of bedding parallel to the bedding of the limestone near by.

(5.) Since ascending lavas have the motion of a fluid, determined partly as to direction of movement by the friction along the sides, a layer of schist 50 or 100 feet long falling into it would not remain entire, and parallel or conformable to the original schistose rock; and much less could a series of such

layers retain such parallelism. Facts like these are not consistent with the theory of an eruptive origin. Moreover the schists are so firm rocks that the separation of layers by such means would be impossible.

I add one additional fact with regard to these large inclusions. In the brownish-black chrysolitic pyroxenyte which occurs along the south side of Montrose Point, there is *a layer of impure, mostly uncrystalline, gray limestone, eighty feet long* (and probably much longer, as this is only the length of the exposure), *and twelve to eighteen inches wide.* It contains some gray-green tremolite or actinolite in the outer portion, and much disseminated pyrite, and owing to the latter is deeply rusted.

It is almost an impossibility that a thin bed of limestone 80 feet long could by any means have got into the erupted rock; and quite impossible that, if in, it should have held together, and retained from one end to the other, even approximately, a uniform strike and dip (N. 12° E., 70° W.).

(6.) At Verplanck Point, where what look like veins or dikes of pyroxenyte occur in the limestone, they are for the most part conformable to the limestone; as if they might be altered beds; and the more northern of these pseudo-veins *consist of mica schist;* further, these pseudo-veins of the Point are represented half a mile northeast in the line of strike by beds of mica schist or hornblendic schist. Such facts appear to show that the most of the "veins" are beds, metamorphosed into different mineral materials according to their varying constitution; and that the contact phenomena manifested are results of the original passage of one rock into the other along the plane of junction and subsequent metamorphic conditions.

In order to appreciate rightly the bearing of the facts on this question as to metamorphism, the mind should be disabused of the common notion that a massive rock, whether feldspathic, hornblendic or augitic, is necessarily of eruptive origin. As heat and moisture may convert siliceous sand-beds, under pressure, into hard *massive* quartzyte without the intervention of fusion, so also it may convert granitic sand-beds into a granite or granite-like rock, as has happened north of Peekskill. Again, the same means, even when the heat is far below that required for fusion, may destroy molecular cohesion, and, as numerous examples show, may convert, by the recrystallization attending metamorphism, well-bedded strata of hornblendic, augitic or feldspathic material into a massive rock, often undistinguishable even microscopically from an eruptive rock. One example in proof is given in my paper in the June number of this Journal (p. 428); and others in papers on the Helderberg rocks of Bernardston, Mass., and Vernon, Vt.* The layer of

* This Journal, III, vi, 339, 1873 and xiv, 379, 1877.

mostly uncrystalline limestone 80 feet long and a foot or more wide in the chrysolitic rock of Montrose Point indicates a temperature of metamorphism much below that of fusion.

3. *Source of the material of the original beds.*

The characteristics of the beds to be accounted for are: (1) the predominance of the magnesian minerals, hornblende, augite, hypersthene, biotite, chrysolite; (2) the abundance of soda-lime feldspars; and (3) the small proportion of free quartz.

The three supposable sources of such characteristics are—

(1) Detritus from the Archæan Highlands.

(2) Igneous eruptions, affording volcanic or igneous debris, in addition to ejected liquid rock, and along with more or less Archæan detritus.

(3) Detritus from the Highlands, supplemented by ingredients from the ocean.

1. Archæan detritus.

The rocks of the Archæan region of the Highlands are largely hornblendic—the gneiss being often a hornblendic gneiss and varying, in many places, to syenyte-gneiss, true syenyte, and hornblende schist; and the mica, whether hornblende is also present or not, is mostly or wholly the black kind, biotite, which, while containing nearly as much potash as muscovite, is characterized by a large percentage of iron and magnesium. Occasionally augitic rocks are present, especially in the vicinity of beds of iron ore. Augitic and hornblendic rocks abound on Anthony's Nose, which is one of the high summits of the Highlands, just to the north of Cortlandt, and they occur less prominently near West Point.

Magnesian as well as ferriferous sediments might therefore have come from such a source; and the frequent occurrence of hornblende schist in regions of the ordinary metamorphic rocks of Westchester County shows that their formation is nothing exceptional. A feeble proportion of free quartz, as in the Cortlandt rocks, is not an uncommon fact. It characterizes muds or clays which have lost their quartz for making sand-beds in the separating process of wave-action or water-movement, and it is exemplified in much hydromica schist, which often consists of hydrous mica alone, with little, if any, free quartz. Again, the soda-lime feldspar, oligoclase, occurs in the granite and gneiss of the Highlands, and, in fact, is common in these rocks wherever found, though in general subordinately to orthoclase; the Cortlandt rocks are peculiar only in the much larger proportion of soda-lime feldspars. In the Archæan of the Adirondacks, labradorite rocks, closely like the noryte and

augite-noryte of Cortlandt in mineral constitution, cover wide regions; and the same kinds may have formerly existed in the Highlands north of the Cortlandt region, although they have not yet been discovered there; and this is somewhat probable, since a drift specimen has been found in central New Jersey, according to Dr. T. Sterry Hunt, and it is not likely that it came from the distant Adirondacks.

Further: chrysolite, although common in igneous rocks, is also common as a metamorphic product, and occurs even in chloritic and mica schist and other rocks, as should be expected from its composition and easy production by heat.

Doubts with regard to Archæan detritus as the only source of these Cortlandt rocks come from the very abrupt transitions which exist between the hornblendic or augitic rocks and the ordinary mica schists and gneiss, so strongly exemplified in the Verplanck region; in the almost exclusive occurrence over so large an area of soda-lime feldspar rocks, when they are not found in a similar way over any other part of Westchester County, the material of whose rocks, the limestones excepted, must have come from the Highlands; the existence of no similar group of rocks in the great central valley of the New Jersey Highlands (that of Greenwood Lake on the map, page 106), or on their western border, where sedimentary beds of Highland origin were extensively formed. The eastern border of the Archæan in New Jersey is under Triassic beds, so that scarcely anything is known of the Lower Silurian strata directly southwest of Stony Point.

2. Igneous ejections along with more or less Archæan detritus.

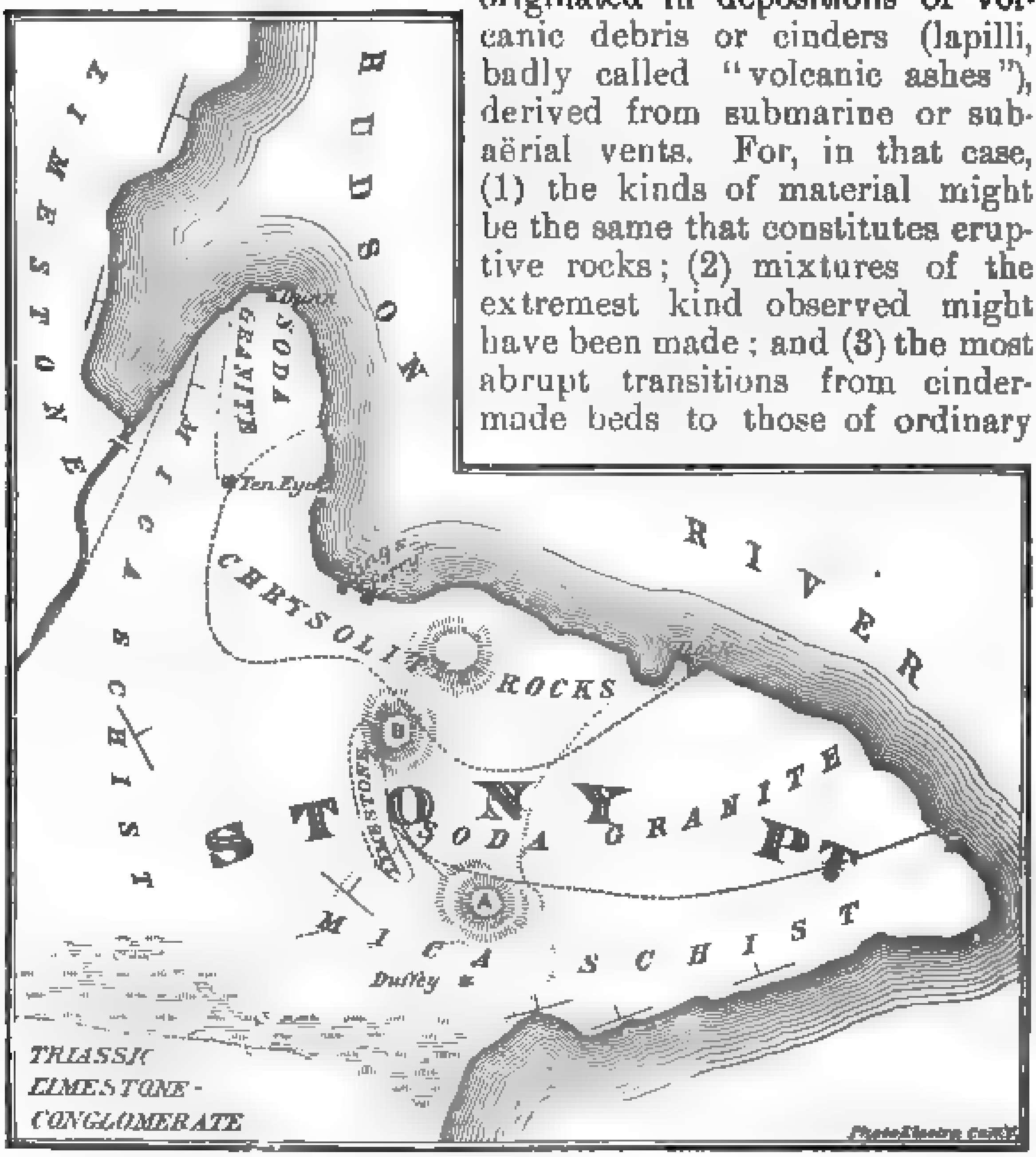
In favor of igneous ejections as a chief source, there are the following facts.

The larger part of the rocks are much like igneous rocks. They resemble them (1) in mineral constitution; (2) in their soda-lime feldspars; (3) in the abundance of hornblende or augite; and (4) in the feeble proportion of quartz. The noryte, though containing hypersthene, offers no objection to the view. The chrysolitic feature of the rocks of some parts of the region is a frequent volcanic characteristic.

But while such resemblances to the igneous rocks exist, it is a striking fact (1) that nowhere in the region are the rocks columnar like those of the Palisades and many regions of augitic igneous rocks; (2) that no vents or dikes have been found to indicate the places of their ejection; (3) that sometimes mixtures of two or three kinds occur—as hornblendyte, pyroxenyte and augite-noryte—which were not combinations made by separate ejections but are merely irregularities of constitution in a single large mass of rock; and occasionally the noryte and

chrysolitic hornblendyte are in united layers each only an inch or two thick; and (4) there are transitions into mica schists not thus easily explained.

These objections appear to prove that the rocks are not truly eruptive. But they do not make it sure that they have not originated in depositions of volcanic debris or cinders (lapilli, badly called "volcanic ashes"), derived from submarine or subaërial vents. For, in that case, (1) the kinds of material might be the same that constitutes eruptive rocks; (2) mixtures of the extremest kind observed might have been made; and (3) the most abrupt transitions from cinder-made beds to those of ordinary sediments might result, even to the intercalation of a layer of limestone or mica schist, or magnetic iron, or emery, besides all degrees of shading from one to the other; moreover (4) the unique character and contracted limits of the area might in this way be fully explained. Such beds of volcanic debris, afterward undergoing metamorphism simultaneously with the general metamorphism of Westchester County strata, would be likely to come out under the various forms and features presented by the rocks described; and even if, in the process, the heat had not reached that of fusion, portions of the beds permeated with heated moisture might have become



plastic and have been injected into fissures so as to produce dike-like veins, and might retain internal marks of their former mobility in broken crystals, if not in other evidences of flowing.

As to the *centers of eruption*, it is to be noted that the occurrence of chrysolitic rocks on both sides of the Hudson—along the shores of Stony Point on the west and of Montrose Point on the east—with noryte adjoining, and next beyond, the soda-granite, may be an indication that one of them was located in what is now the river channel off the Verplanck shores. (See map, p. 107).

Since my former account of Stony Point was published I have made a further examination of the region with reference to this and other points. The chief facts as to the distribution and positions of the rocks are given in the preceding map.* The *mica schist* of the northwest and south sides of the Point join over the southwestern side; and the strike and dip show that there is here one stratum in a *synclinal* fold. Overlying the schist occurs the *soda-granite* in two areas; and next comes the *chrysolitic rocks*. The chrysolitic rocks thus occupy approximately the middle portion of the synclinal.†

The soda-granite is mostly of the coarsely crystalline, light-colored kind, looking like ordinary granite, but, in the vicinity of the schist, in some parts, a fine-grained variety, gray to black in color, occurs; and the fine variety sometimes intersects the coarse, or the reverse, as if in veins. In one case, near the

* This map is based on a survey of the Point by Mr. L. Wilson, Principal of the Mountain Institute, Haverstraw, N. Y., obligingly made at the request of the writer.

† In my former account of the Point, I showed that the Tompkins Cove limestone stratigraphically underlies the mica schist, it dipping under it, as at Cruger's; and the more recent examination confirms this conclusion. It is therefore probable that the stratum to the north of the Point bends around following the flexure of the schist; and that it lies beneath the area of Triassic conglomerate, and thence extends eastward along the bed of the Hudson.

It is a fact of interest that at Cruger's this overlying schist is *fibrolitic*, just like the overlying gneiss adjoining the limestone of New York Island. The fibrolitic gneiss of 123d street, on the corner of Lexington avenue, is but a few yards from the limestone.

In the interior of the peninsula between the schist and the granite, but quite near the junction with the chrysolitic rock, occurs a thick stratum of limestone (see map, p. 112). It is about conformable to the schist on the south of it, but stops off to the northward with a nearly vertical dip (70°–80° N.) and a strike of N. 70° E. The limestone is situated somewhat like the small beds in the interior of the Verplanck peninsula, and as near to the massive rock; the latter was proved in one case to be conformable to planes of bedding in the neighboring noryte; and in another case, to the mica schist; but the relations of this Stony Point bed to the massive rocks I could not determine. As in Verplanck it is probably a distinct stratum from that of Tompkins Cove; it is semi-crystalline like that, while other parts are coarsely granular, tremolitic and somewhat garnetiferous.

The Tompkins Cove limestone, on the shore just north of the limits of the above map contains many veins of quartz, and assays made for the proprietor, Mr. Edward A. Swain, have proved that the quartz is auriferous.

southern entrance to the grounds a dike (or *vein*) two feet wide, of the black micaceous variety, intersects the *mica schist* cutting obliquely across its bedding.

The direct contact of the granite and chrysolitic rocks is no where in sight. But where the granite ends near the chrysolitic rocks it stands in a nearly vertical wall, having approximately the same strike and dip as the schists to the southeast. The position of the chrysolitic rocks suggests an igneous origin.

With regard to the *time of the ejections*, supposing these a fact, the evidence stands as follows:

The hornblendic and augitic materials occur in conformable beds in the limestone of Verplanck point, looking like dikes or veins because now nearly vertical, as has been explained; and hence this material must have been supplied when the limestone was forming; and the limestone is part of the same stratum, as has been shown, with that of Canopus Hollow, Tompkins Cove and Cruger's Station. Moreover, the dip of the beds seem to indicate that these rocks *overlie* the limestone of the region. Hence the eruptions were in progress while the limestone was forming, and continued on for a period after it.

It may be objected to this view of an igneous source that the chrysolitic pyroxenyte and hornblendyte are very unlike ordinary igneous chrysolitic rocks, the chrysolite never being in glassy grains; that chrysolitic pyroxenyte, though a known kind, is not in all cases igneous; and that chrysolitic hornblendyte like that here met with (having hornblende cleavage faces measuring sometimes two inches each way) is still less like an igneous product. So, also, soda-granite is a very unusual form of eruptive rock, and likewise dioryte with crystals of hornblende sometimes eight or nine inches long, like that near Cruger's. But these difficulties, and others like them, lose much of their force in view of the fact that the beds may contain more or less ordinary detritus, as well as volcanic debris, and especially the other fact that they have undergone metamorphism since their deposition, and in some cases have thereby suffered partial or complete fusion.

Again, it may be urged in objection that we have no definite evidence as to the former existence of such a vent in the channel of the Hudson, or of any other in the region. This objection may hereafter be strengthened, or, on the other hand, weakened, by finding that among the dikes of igneous rocks which intersect the Archæan in various places, some, or none, consist of rocks similar to those of Cortlandt.

Professor Cook, in his Geological Report of 1868, at page 144, has described a labradorite rock, resembling somewhat the

Cortlandt noryte or augite-noryte, as occurring forty miles west of the Hudson on the east slope of the Kittatinny Mountains, not far west of Libertyville and Deckertown (between *c* and *d* on the map, Plate IX, in this Journal for last November); and he speaks of it as constituting a dike a fourth of a mile wide and several miles long, coming in between the Hudson River slate and the overlying Oneida Conglomerate, and conforming to them in strike. In a recent letter to the writer he observes that the question as to whether eruptive or not he does not consider as settled, the debris of the region having prevented satisfactory examination. The adjoining slates are stated to be modified, as if from the influence of the mass, for 3,000 feet to the eastward—a distance so great that the effects can hardly be all due to contact. The further study of that region may throw light on the Cortlandt rocks.

(3.) ARCHÆAN DETRITUS, SUPPLEMENTED BY MATERIALS FROM THE OCEAN.

The chief stony materials which the ocean's waters have to contribute are: (1) the *calcareous*—calcium carbonate mainly through the secretions (shells, corals, etc.) of its living species, and calcium chloride; (2) the *magnesian*, from the magnesium chloride and sulphate; and (3) the *soda*, through the sodium chloride or common salt.

The calcareous and magnesian materials of the oceanic waters have been of immense importance in rock-making. The limestones of the world have originated from the former. Besides this, few muds or argillaceous sand beds have been made since the first Rhizopods appeared that have not contained more or less disseminated calcareous material; and this material, in the course of the metamorphism of those beds, has been often employed in producing some of the new combinations constituting metamorphic rocks. So, also, the ocean has been the chief source of the magnesia used for making dolomite, or magnesian limestone, and for other purposes. In the case of the limestone of Westchester County, which is dolomitic, the magnesia was taken from the sea-water, according to the most generally accepted view, while the process of consolidation was going on in great marshes of concentrated saline waters.

Further, when the magnesian limestones thus made were afterward rendered metamorphic, part of the magnesia and lime (or magnesium and calcium) was in many cases made into silicates, such as tremolite, white pyroxene, and other species; or, when iron has also been present, into other related silicates of light or dark green tints, as hornblende, actinolite, green pyroxene; and also into other magnesian minerals through other impurities of the limestone.

Thus the magnesia of the ocean's waters has beyond doubt

supplemented that of detritus in determining the constitution of metamorphic rocks, and has led especially to the production of different varieties of hornblende and pyroxene, the darker kinds resulting when the all-pervading ingredient, iron, was present.

Further, the ocean has been one of the sources of soda in rock-making. The contributions of this nature to sedimentary deposits, are, as is well known, common and extensive. Beds of rock salt, sometimes of great thickness, occur in formations of various ages, from the Silurian to the present time; and magnesian salts, derived, directly or indirectly, from the same sea-waters that afforded the rock salt, are also frequently present.

Moreover, brines from deep borings are common. It is not necessary here to give details. I mention two American cases only, one relating to the Lower Silurian formation, and the other pertaining to the vicinity of the region under discussion.

The boring at the St. Louis Insane Asylum, reported upon by Mr. G. C. Broadhead, State Geologist of Missouri,* which penetrated through Carboniferous and Lower Silurian strata into the Archæan, reached a depth of 3,843½ feet. "Salt water" was obtained in the Lower Silurian (Magnesian limestone) at a depth of 1,220 feet and below. At 2,256 feet, the water contained 3 per cent of salt; at 2,957 feet, 4½ per cent; at 3,293 feet, 2 per cent; and below 3,545 feet, 7 to 8 per cent.

Prof. G. H. Cook, State Geologist of New Jersey, states in his Report for the year 1880, that from a boring in the Triassic sandstone at Patterson in that State (which is in the same geographical region with the Cortlandt area, it lying to the east of the Archæan Highlands) the water obtained at 2,050 feet afforded, per gallon, 408·46 grains of sodium chloride, with 109·44 of magnesium chloride and 278·32 of calcium chloride —which shows the presence of about half the proportion of salt contained in sea-water, and of a much larger proportion of magnesium and calcium chlorides than sea water contains; and Prof. Cook adds: "the questions suggested by finding the salt water must remain for the present unanswered, though the fact that the rock-salt of Europe is found in rocks of the same age as this raises the question whether it may not also be found here."

Rocks containing salt in beds or brines have undoubtedly undergone metamorphism, and under conditions as to superincumbent formations which permitted of no escape of the sodium, and which therefore would have forced it into chemical combination with the other materials present. And if it has entered into any minerals the feldspars must be among them,

* Report on the Geological Survey of Missouri for 1873–1874, 8vo, p. 32. 1874.

since these are the commonest of anhydrous sodium silicates. Science looks to the ocean for the boric acid of some minerals and the chlorine and iodine of certain silver ores and some volcanic products; and hence referring to it as a source of the more stable bases with which these were combined is not unreasonable.

In Savoy, as has long been known, the crystalline magnesian limestone or dolomite of the Trias contains the soda-feldspar, albite, in disseminated crystals. The magnesia of the limestone must have come from evaporated sea-water as above explained; and the soda of the feldspar which, in the metamorphic process that crystallized the dolomite, went to make albite may have had the same source.

Messrs. F. Fouqué and Michel Lévy have recently made* crystallized oligoclase and labradorite by heating a mixture of silica, alumina (each of these in the states obtained by precipitation), sodium carbonate and calcium carbonate in the required proportions, and keeping it in prolonged fusion. They have thus proved that the sodium of a sodium carbonate will, at a high temperature, enter into combination and make feldspars. The sodium of sodium chloride (common salt) would in all probability yield the same result; as is indicated by the use of common salt in putting a glazing on porcelain (while it is at a high heat) the chlorine escaping and yielding the sodium to make a silicate, or the glaze. The possibility of producing soda-lime feldspar in the metamorphism of a saliferous sedimentary stratum has therefore been put beyond question by actual experiment. Metamorphic heat would be as effectual; and, with the aid of moisture, probably at a lower temperature than that employed by Fouqué.

Crystalline rocks made largely of soda-lime feldspars,—some of which are diorite, noryte, and the labradorite rocks called gabbro—covering many large regions, are in some cases unquestionably of metamorphic origin; and if detritus from preexisting rocks were not a sufficient source for the soda of the feldspars and the magnesia of the hornblende or augite, and a volcanic or igneous source is not indicated by surrounding conditions, there must have been at hand some other large and abundant source; and the universal ocean is of just the kind needed. Near New Haven, Connecticut, a chloritic hydromica schist contains, along a certain horizon, interrupted beds or lenticular masses of limestone—parts of which are more or less changed to serpentine and verd-antique marble; and below the limestone horizon, the schist, for a considerable thickness, contains irregular masses of labradioryte (labradorite-dioryte), the slaty-beds of the schist changing for short distances to labra-

* Comptes Rendus, vol. lxxxvii, pp. 700 and 779, November, 1878.

dioryte and then back again to slate, in the most irregular way. The idea of an eruptive origin is utterly out of the question; and that of a "volcanic-ash" origin for the material has nothing to sustain it, since not even one small dike of igneous rock or any other evidence of igneous eruption older than Triassic has yet been found within a circuit of fifty miles; and what there are of veins in the older rocks are made of granitic or siliceous material. Since these isolated portions of massive labradioryte are parts of a stratum lying directly beneath the limestone horizon, which stratum would be likely to be more or less calcareous through an organic source, the lime of the labradorite in this rock may be only the calcareous portion of the original sediments; and what additional soda was needed may have come from the permeating brine water. This example may illustrate the mode of origin of other metamorphic labradorite and oligoclase rocks.

The hypothesis that the massive Cortlandt rocks were made by the above-explained method—that is from "ordinary detritus supplemented by materials from the ocean"—is therefore not wholly improbable. It is still less so when some details connected with it are considered.

The position of the area—in the angle between the New Jersey and Putnam County Highlands, the site of a Lower Silurian bay—was favorable for the occurrence of the required conditions. The limestone (dolomite) shows, by its magnesia, that during the long era in which it was accumulating from the organic secretions of the waters, evaporating brine-making sea-marshes prevailed, or alternated with open seas, over the shallow bay. The beds of fine mica schist, one to ten feet thick, which occur intercalated in the limestone, northeast of Verplanck Point, show that the sea-marshes in some parts became covered at intervals with mud-deposits containing (as the black mica proves, and also the hornblende and augite present in some of the schist) iron oxide and magnesia. And, finally, the occurrence just southwest, at Verplanck Point, in the same limestone, of conformable intercalations of noryte, pyroxenyte and hornblendyte—the massive Cortlandt rocks containing little of the black mica—and, by the side of these, some true mica schist beds, accords with the view that in this part of the area the depositions of common and magnesian salts from the marsh were at some horizons of the detritus more abundant than to the northeast. The nearly total absence of free silica may have its explanation also in these conditions, since the bases contributed by the sea-water, the soda, magnesia and lime, together with the iron from outside sources, would have needed it to make the silicates. If these are the right explanations for the facts at Verplanck Point, the principle is

equally good for all in the Cortlandt and Stony Point area, and for all variations in the kinds and the thicknesses of the rocks, and their intercalations. Whether true or not, it must, after the survey of the facts, be admitted to be nothing against it that the rocks are massive crystalline rocks; that among them are hornblendic and augitic kinds containing soda-lime feldspars, and that some of them are chrysolitic.

Having presented the claims of the three hypotheses, I leave the subject without expressing a personal opinion.

The Appendix to this memoir, to which allusion has been made, will appear in a following number of this Journal.

ART. XX.—*On a new Meteoric Iron, of unknown locality, in the Smithsonian Museum;* by CHARLES UPHAM SHEPARD.

HAVING received a fragment from a meteoric iron, of unknown locality, belonging to the Museum of the Smithsonian Institution, I have made an examination of it with the following results:

The mass was oval in form, with three or four prominent knobs. Its weight was probably about six pounds. The fragment for examination was separated with considerable facility, requiring only a few smart blows of the hammer; and revealed a crystalline structure. The surfaces developed were partially covered by an exceedingly thin, micaceous layer of schreibersite. After polishing, the fragment had a somewhat whiter color than artificial iron. When etched, it showed a homogeneous, finely crystalline texture, and became still whiter in color. When viewed at fixed angles of reflexion, the surface glimmered simultaneously, after the manner of sunstone oligoclasite, thus rendering it probable that the crystallization of the general mass was that of a single individual.

It is obscurely banded, in some portions, with bars about $\frac{1}{20}$th of an inch in thickness. But the most remarkable feature of the etched surface is its thickly dotted or punctate character; the dots which are very bright, instead of being salient points, are slightly concave. On the whole, therefore, this iron differs in structure from any meteoric iron thus far known. The composition, as determined by C. U. Shepard, Jr., is

Iron	92·923	
Nickel	6·071	
Cobalt	0·539	
Schreibersite (phosphide of iron)	0·562	—100·095

There are traces also of copper and tin. The polished surfaces show no tendeny to deliquescence. Sp. gr.=7·589.

Charleston, Feb. 19, 1881.

ART. XXI.—*The relative motion of the Earth and the Luminiferous ether*; by ALBERT A. MICHELSON, Master, U. S. Navy.

THE undulatory theory of light assumes the existence of a medium called the ether, whose vibrations produce the phenomena of heat and light, and which is supposed to fill all space. According to Fresnel, the ether, which is enclosed in optical media, partakes of the motion of these media, to an extent depending on their indices of refraction. For air, this motion would be but a small fraction of that of the air itself and will be neglected.

Assuming then that the ether is at rest, the earth moving through it, the time required for light to pass from one point to another on the earth's surface, would depend on the direction in which it travels.

Let V be the velocity of light.

v = the speed of the earth with respect to the ether.

D = the distance between the two points.

d = the distance through which the earth moves, while light travels from one point to the other.

d_1 = the distance earth moves, while light passes in the opposite direction.

Suppose the direction of the line joining the two points to coincide with the direction of earth's motion, and let T = time required for light to pass from the one point to the other, and T_1 = time required for it to pass in the opposite direction. Further, let T_0 = time required to perform the journey if the earth were at rest.

$$\text{Then } T=\frac{D+d}{V}=\frac{d}{v};\ \text{and } T_1=\frac{D-d}{V}=\frac{d_1}{v}$$

From these relations we find $d=D\frac{v}{V-v}$ and $d_1=D\frac{v}{V+v}$ whence $T=\frac{D}{V-v}$ and $T_1=\frac{D}{V+v}$; $T-T_1=2T_0\frac{v}{V}$ *nearly*, and $v=V\frac{T-T_1}{2T_0}$.

If now it were possible to measure $T-T_1$ since V and T_0 are known, we could find v the velocity of the earth's motion through the ether.

In a letter, published in "Nature" shortly after his death, Clerk Maxwell pointed out that $T-T_1$ could be calculated by measuring the velocity of light by means of the eclipses of Jupiter's satellites at periods when that planet lay in different directions from earth; but that for this purpose the observations of these eclipses must greatly exceed in accuracy those

which have thus far been obtained. In the same letter it was also stated that the reason why such measurements could not be made at the earth's surface was that we have thus far no method for measuring the velocity of light which does not involve the necessity of returning the light over its path, whereby it would lose nearly as much as was gained in going. The difference depending on the square of the ratio of the two velocities, according to Maxwell, is far too small to measure.

The following is intended to show that, with a wave-length of yellow light as a standard, the quantity—if it exists—is easily measurable.

Using the same notation as before we have $T=\frac{D}{V-v}$ and $T_1=\frac{D}{V+v}$. The whole time occupied therefore in going and returning $T+T_1=2D\frac{V}{V^2-v^2}$. If, however, the light had traveled in a direction at right angles to the earth's motion it would be entirely unaffected and the time of going and returning would be, therefore, $2\frac{D}{V}=2T_0$. The difference between the times $T+T_1$ and $2T_0$ is

$$2DV\left(\frac{1}{V^2-v^2}-\frac{1}{V^2}\right)=\tau\,;\ \tau=2DV\frac{v^2}{V^2(V^2-v^2)}$$

or nearly $2T_0\frac{v^2}{V^2}$. In the time τ the light would travel a distance $V\tau=2VT_0\frac{v^2}{V^2}=2D\frac{v^2}{V^2}$.

That is, the actual distance the light travels in the first case is greater than in the second, by the quantity $2D\frac{v^2}{V^2}$.

Considering only the velocity of the earth in its orbit, the ratio $\frac{v}{V}=\frac{1}{10\,000}$ approximately, and $\frac{v^2}{V^2}=\frac{1}{100\,000\,000}$. If $D=$ 1200 millimeters, or in wave-lengths of yellow light, 2 000 000, then in terms of the same unit, $2D\frac{v^2}{V^2}=\frac{4}{100}$.

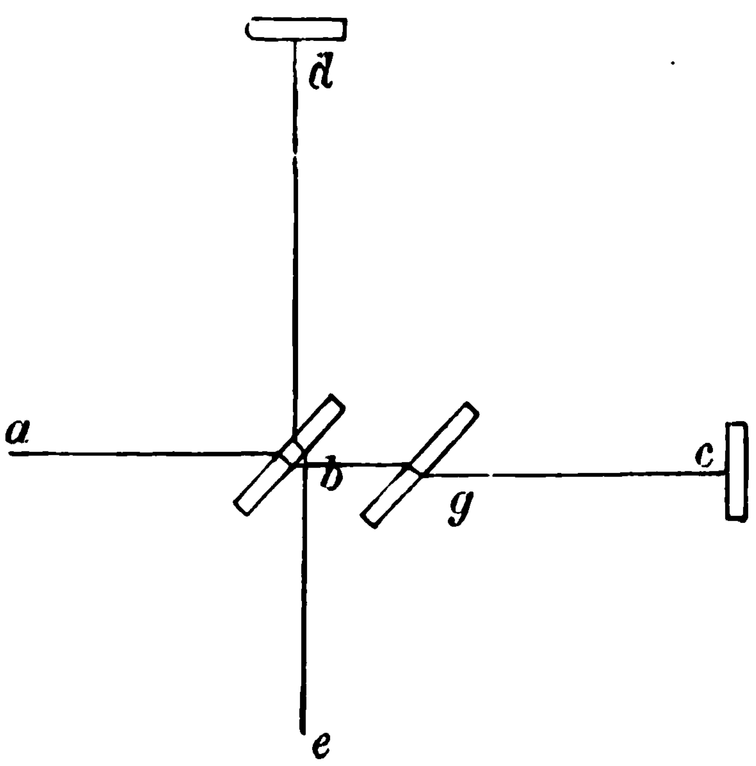
If, therefore, an apparatus is so constructed as to permit two pencils of light, which have traveled over paths at right angles to each other, to interfere, the pencil which has traveled in the direction of the earth's motion, will in reality travel $\frac{4}{100}$ of a wave-length farther than it would have done, were the earth at rest. The other pencil being at right angles to the motion would not be affected.

If, now, the apparatus be revolved through 90° so that the second pencil is brought into the direction of the earth's motion, its path will have lengthened $\frac{4}{100}$ wave-lengths. The total change in the position of the interference bands would be $\frac{8}{100}$ of the distance between the bands, a quantity easily measurable.

The conditions for producing interference of two pencils of light which had traversed paths at right angles to each other were realized in the following simple manner.

Light from a lamp *a*, fig. 1, passed through the plane parallel glass plate *b*, part going to the mirror *c*, and part being reflected to the mirror *d*. The mirrors *c* and *d* were of plane glass, and silvered on the front surface. From these the light was reflected to *b*, where the one was reflected and the other refracted, the two coinciding along *be*.

1.



The distance *bc* being made equal to *bd*, and a plate of glass *g* being interposed in the path of the ray *bc*, to compensate for the thickness of the glass *b*, which is traversed by the ray *bd*, the two rays will have traveled over equal paths and are in condition to interfere.

The instrument is represented in plan by fig. 2, and in perspective by fig. 3. The same letters refer to the same parts in the two figures.

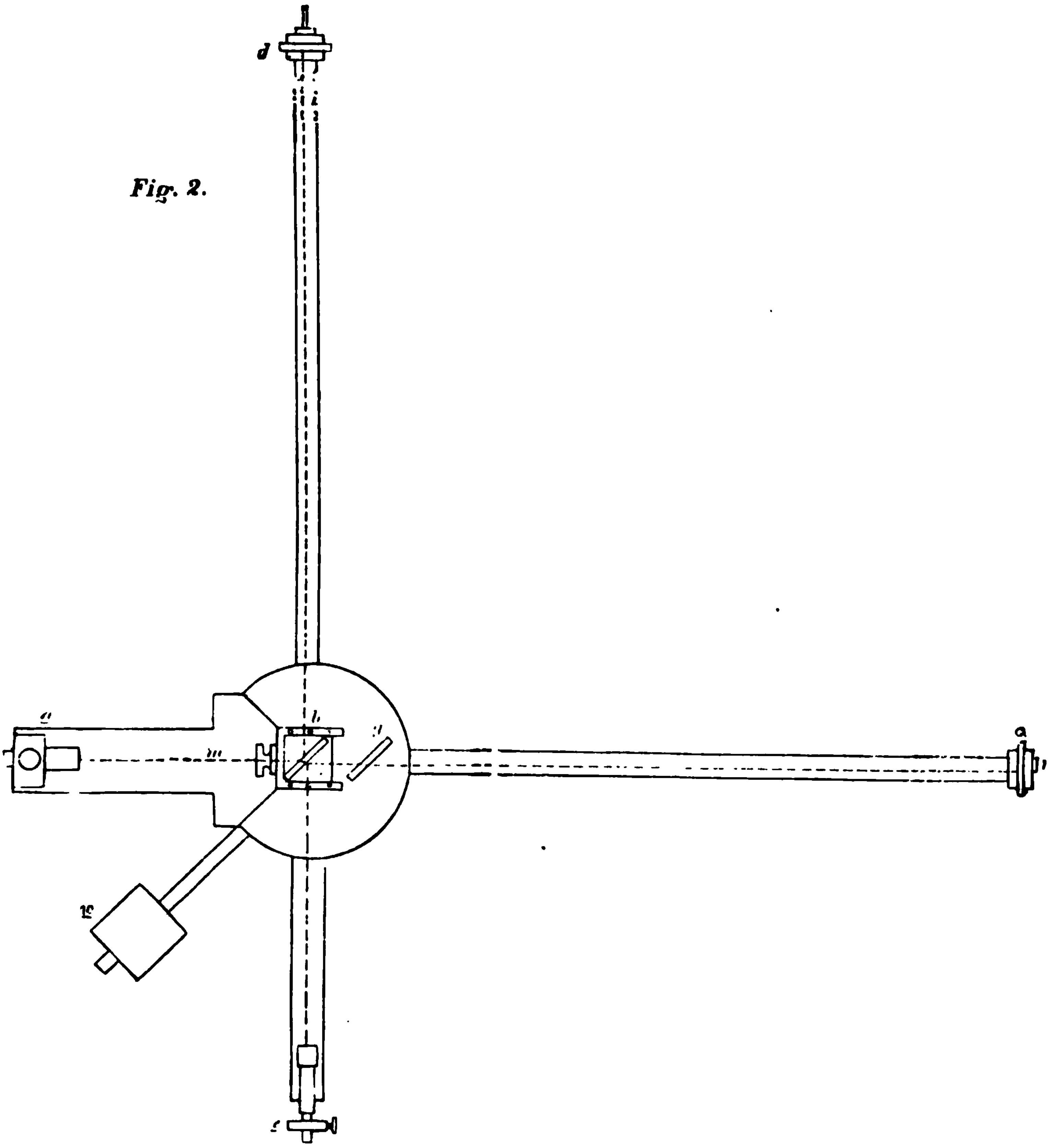
The source of light, a small lantern provided with a lens, the flame being in the focus, is represented at *a*. *b* and *g* are the two plane glasses, both being cut from the same piece; *d* and *c* are the silvered glass mirrors; *m* is a micrometer screw which moves the plate *b* in the direction *bc*. The telescope *e*, for observing the interference bands, is provided with a micrometer eyepiece. *w* is a counterpoise.

In the experiments the arms, *bd*, *bc*, were covered by long paper boxes, not represented in the figures, to guard against changes in temperature. They were supported at the outer ends by the pins *k*, *l*, and at the other by the circular plate *o*. The adjustments were effected as follows:

The mirrors *c* and *d* were moved up as close as possible to the plate *b*, and by means of the screw *m* the distances between a point on the surface of *b* and the two mirrors were made approximately equal by a pair of compasses. The lamp being

lit, a small hole made in a screen placed before it served as a point of light; and the plate *b*, which was adjustable in two planes, was moved about till the two images of the point of light, which were reflected by the mirrors, coincided. Then a sodium flame placed at *a* produced at once the interference bands. These could then be altered in width, position, or direction, by a slight movement of the plate *b*, and when they were of convenient width and of maximum sharpness, the

Fig. 2.



sodium flame was removed and the lamp again substituted. The screw *m* was then slowly turned till the bands reappeared. They were then of course colored, except the central band, which was nearly black. The observing telescope had to be focussed on the surface of the mirror *d*, where the fringes were most distinct. The whole apparatus, including the lamp and the telescope, was movable about a vertical axis.

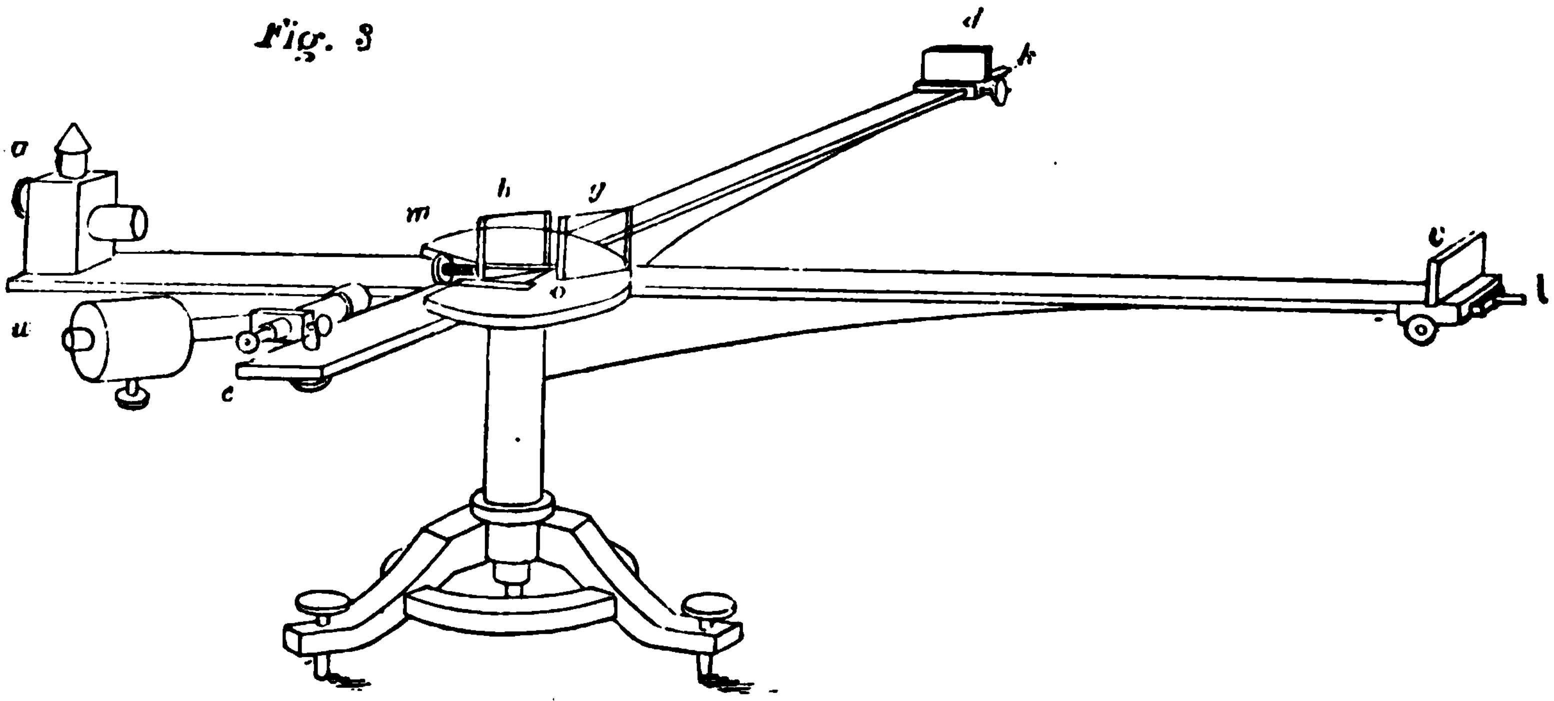
It will be observed that this apparatus can very easily be

made to serve as an "interferential refractor," and has the two important advantages of small cost, and wide separation of the two pencils.

The apparatus as above described was constructed by Schmidt and Hænsch of Berlin. It was placed on a stone pier in the Physical Institute, Berlin. The first observation showed, however, that owing to the extreme sensitiveness of the instrument to vibrations, the work could not be carried on during the day. The experiment was next tried at night. When the mirrors were placed half-way on the arms the fringes were visible, but their position could not be measured till after twelve o'clock, and then only at intervals. When the mirrors were moved out to the ends of the arms, the fringes were only occasionally visible.

It thus appeared that the experiments could not be performed in Berlin, and the apparatus was accordingly removed

Fig. 3



to the *Astrophysicalisches Observatorium* in Potsdam. Even here the ordinary stone piers did not suffice, and the apparatus was again transferred, this time to a cellar whose circular walls formed the foundation for the pier of the equatorial.

Here, the fringes under ordinary circumstances were sufficiently quiet to measure, but so extraordinarily sensitive was the instrument that the stamping of the pavement, about 100 meters from the observatory, made the fringes disappear entirely!

If this was the case with the instrument constructed with a view to avoid sensitiveness, what may we not expect from one made as sensitive as possible!

At this time of the year, early in April, the earth's motion in its orbit coincides roughly in longitude with the estimated direction of the motion of the solar system—namely, toward the constellation Hercules. The direction of this motion is inclined at an angle of about +26° to the plane of the equator,

and at this time of the year the tangent of the earth's motion in its orbit makes an angle of $-23\frac{1}{2}°$ with the plane of the equator; hence we may say the resultant would lie within 25° of the equator.

The nearer the two components are in magnitude to each other, the more nearly would their resultant coincide with the plane of the equator.

In this case, if the apparatus be so placed that the arms point north and east at noon, the arm pointing east would coincide with the resultant motion, and the other would be at right angles. Therefore, if at this time the apparatus be rotated 90°, the displacement of the fringes should be *twice* $\frac{8}{100}$ or 0·16 of the distance between the fringes.

If, on the other hand, the proper motion of the sun is small compared to the earth's motion, the displacement should be $\frac{6}{10}$ of ·08 or 0·048. Taking the mean of these two numbers as the most probable, we may say that the displacement to be looked for is not far from one-tenth the distance between the fringes.

The principal difficulty which was to be feared in making these experiments, was that arising from changes of temperature of the two arms of the instrument. These being of brass whose coefficient of expansion is 0·000019 and having a length of about 1000 mm. or 1 700 000 wave-lengths, if one arm should have a temperature only one one-hundredth of a degree higher than the other, the fringes would thereby experience a displacement three times as great as that which would result from the rotation. On the other hand, since the changes of temperature are independent of the direction of the arms, if these changes were not too great their effect could be eliminated.

It was found, however, that the displacement on account of bending of the arms during rotation was so considerable that the instrument had to be returned to the maker, with instructions to make it revolve as easily as possible. It will be seen from the tables, that notwithstanding this precaution a large displacement was observed in one particular direction. That this was due entirely to the support was proved by turning the latter through 90°, when the direction in which the displacement appeared was also changed 90°.

On account of the sensitiveness of the instrument to vibration, the micrometer screw of the observing telescope could not be employed, and a scale ruled on glass was substituted. The distance between the fringes covered three scale divisions, and the position of the center of the dark fringe was estimated to fourths of a division, so that the separate estimates were correct to within $\frac{1}{12}$.

It frequently occurred that from some slight cause (among

others the springing of the tin lantern by heating) the fringes would suddenly change their position, in which case the series of observations was rejected and a new series begun.

In making the adjustment before the third series of observations, the direction in which the fringes moved, on moving the glass plate b, was reversed, so that the displacement in the third and fourth series are to be taken with the opposite sign.

At the end of each series the support was turned 90°, and the axis was carefully adjusted to the vertical by means of the foot-screws and a spirit level.

	N.	N.E.	E.	S.E.	S.	S.W.	W.	NW.	Remarks.
1st revolution	0·0	0·0	0·0	−8·0	−1·0	−1·0	−2·0	3·0	Series 1, footscrew marked B, toward East.
2d "	16·0	16·0	16·0	9·0	16·0	16·0	15·0	13·0	
3d "	17·0	17·0	17·0	10·0	17·0	16·0	16·0	17·0	
4th "	15·0	15·0	15·0	8·0	14·5	14·5	14·5	14·0	
5th "	13·5	13·5	13·5	5·0	12·0	13·0	13·0	13·0	
	61·5	61·5	61·5	x	58·5	58·5	56·5	54·0	
S.	58·5	W.	56·5		N.E.	61·5	S.E.	60·0	
	120·0		118·0			120·0		114·0	
	118·0					114·0			
Excess,	+2·0					+6·0			
1st revolution	10·0	11·0	12·0	13·0	13·0	0·0	14·0	15·0	Series 2, B toward South.
2d "	16·0	16·0	16·0	17·0	17·0	2·0	17·0	17·0	
3d "	17·5	17·5	17·5	17·5	17·5	4·0	18·0	17·5	
4th "	17·5	17·5	17·0	17·0	17·0	4·0	17·0	17·0	
5th "	17·0	17·0	17·0	17·0	16·0	3·0	16·0	16·0	
	78·0	79·0	79·5	81·5	80·5	x	82·0	82·5	
S.	80·5	W.	82·0		N.E.	79·0	S.E.	81·5	
	158·5		161·5			160·0		164·0	
	161·5					164·0			
Excess,	−3·0					−4·0			
1st revolution	3·0	3·0	3·0	3·0	2·5	2·5	2·5	10·0	Series 3. B toward West.
2d "	18·0	17·5	17·5	18·0	18·5	19·0	19·5	26·0	
3d "	11·0	11·0	13·0	12·0	13·0	13·5	13·5	21·0	
4th "	1·0	0·0	0·5	0·5	0·5	0·0	0·0	14·0	
5th "	4·0	4·0	5·0	5·0	5·0	5·5	5·5	16·0	
	37·0	35·5	39·0	38·5	39·5	40·5	71·0	x	
S.	39·5	W.	41·0		N.E.	35·5	S.E.	38·5	
	76·5		80·0			76·0		79·5	
			76·5					76·0	
Excess,			+3·5					+3·5	
1st revolution.	14·0	21·0	15·5	17·0	14·0	14·5	14·5	16·0	Series 4, B toward North.
2d "	10·0	20·0	12·0	12·0	13·0	13·0	13·0	13·5	
3d "	14·0	25·0	15·0	16·0	16·0	16·0	16·0	17·0	
4th "	18·0	27·0	18·5	18·5	18·5	19·0	20·0	21·0	
5th "	15·0	24·0	15·0	15·0	15·0	16·0	16·0	16·5	
	71·0	x	76·0	78·5	76·5	78·5	79·5	84·0	
S.	76·5	W.	79·5		N.E.	73·5	S.E.	78·5	
	147·5		155·5			152·0		162·5	
			147·5					152·0	
Excess,			+8·0					+10·5	

The heading of the columns in the table gives the direction toward which the telescope pointed.

The footing of the erroneous column is marked x, and in the calculations the mean of the two adjacent footings is substituted.

The numbers in the columns are the positions of the center of the dark fringe in *twelfths* of the distance between the fringes.

In the first two series, when the footings of the columns N. and S. exceed those of columns E. and W., the excess is called positive. The excess of the footings of N.E., S.W., over those of N.W., S.E., are also called positive. In the third and fourth series this is reversed.

The numbers marked "excess" are the sums of ten observations. Dividing therefore by 10, to obtain the mean, and also by 12 (since the numbers are twelfths of the distance between the fringes), we find for

	N.S.	N.E., S.W.
Series 1	+0·017	+0·050
" 2	−0·025	−0·033
" 3	+0·030	+0·030
" 4	+0·067	+0·087
	4) 0·089	0·137
Mean =	+0·022	+0·034

The displacement is, therefore,

In favor of the columns N.S.	+0·022
" " " N.E., S.W.	+0·034

The former is too small to be considered as showing a displacement due to the simple change in direction, and the latter should have been zero.

The numbers are simply outstanding errors of experiment. It is, in fact, to be seen from the footings of the columns, that the numbers increase (or decrease) with more or less regularity from left to right.

This gradual change, which should not in the least affect the periodic variation for which we are searching, would of itself necessitate an outstanding error, simply because the sum of the two columns farther to the left must be less (or greater) than the sum of those farther to the right.

This view is amply confirmed by the fact that where the excess is positive for the column N.S., it is also positive for N.E., S.W., and where negative, negative. If, therefore, we can eliminate this gradual change, we may expect a much smaller error. This is most readily accomplished as follows:

Adding together all the footings of the four series, the third and fourth with negative sign, we obtain

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
31·5	31·5	26·0	24·5	23·0	20·8	18·0	11·0

or dividing by 20×12 to obtain the means in terms of the distance between the fringes,

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
0·131	0·131	0·108	0·102	0·096	0·086	0·075	0·046

If x is the number of the column counting from the right and y the corresponding footing, then the method of least squares gives as the equation of the straight line which passes nearest the points x, y—

$$y = 9{\cdot}25x + 64{\cdot}5$$

If, now, we construct a curve with ordinates equal to the difference of the values of y found from the equation, and the actual value of y, it will represent the displacements observed, freed from the error in question.

These ordinates are:

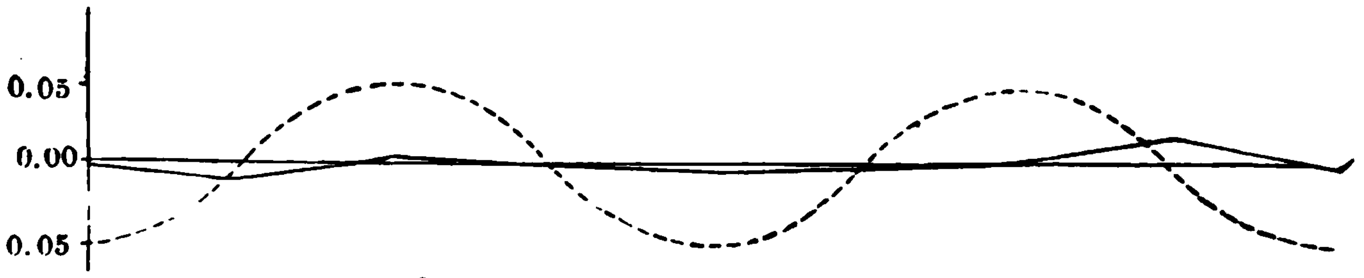
N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
−·002	−·011	+·003	−·001	−·004	−·003	−·001	+·018

N.	−·002	E.	+·003	N.E.	−·011	N.W.	+·018
S.	−·004	W.	−·001	S.W.	−·003	S.E.	−·001
Mean=	−·003		+·001	Mean=	−·007		+·008
	+·001				+·008		
Excess=	−·004			Excess=	−·015		

The small displacements $-0{\cdot}004$ and $-0{\cdot}015$ are simply errors of experiment.

The results obtained are, however, more strikingly shown by constructing the actual curve together with the curve that should have been found if the theory had been correct. This is shown in fig. 4.

4.



The dotted curve is drawn on the supposition that the displacement to be expected is one-tenth of the distance between the fringes, but if this displacement were only $\frac{1}{100}$, the broken line would still coincide more nearly with the straight line than with the curve.

The interpretation of these results is that there is no displacement of the interference bands. The result of the hypothesis of a stationary ether is thus shown to be incorrect, and the necessary conclusion follows that the hypothesis is erroneous.

This conclusion directly contradicts the explanation of the phenomenon of aberration which has been hitherto generally accepted, and which presupposes that the earth moves through the ether, the latter remaining at rest.

It may not be out of place to add an extract from an article published in the Philosophical Magazine by Stokes in 1846.

"All these results would follow immediately from the theory of aberration which I proposed in the July number of this magazine: nor have I been able to obtain any result admitting of being compared with experiment, which would be different according to which theory we adopted. This affords a curious instance of two totally different theories running parallel to each other in the explanation of phenomena. I do not suppose that many would be disposed to maintain Fresnel's theory, when it is shown that it may be dispensed with, inasmuch as we would not be disposed to believe, without good evidence, that the ether moved quite freely through the solid mass of the earth. Still it would have been satisfactory, if it had been possible to have put the two theories to the test of some decisive experiment."

In conclusion, I take this opportunity to thank Mr. A. Graham Bell, who has provided the means for carrying out this work, and Professor Vogel, the Director of the *Astrophysicalisches Observatorium*, for his courtesy in placing the resources of his laboratory at my disposal.

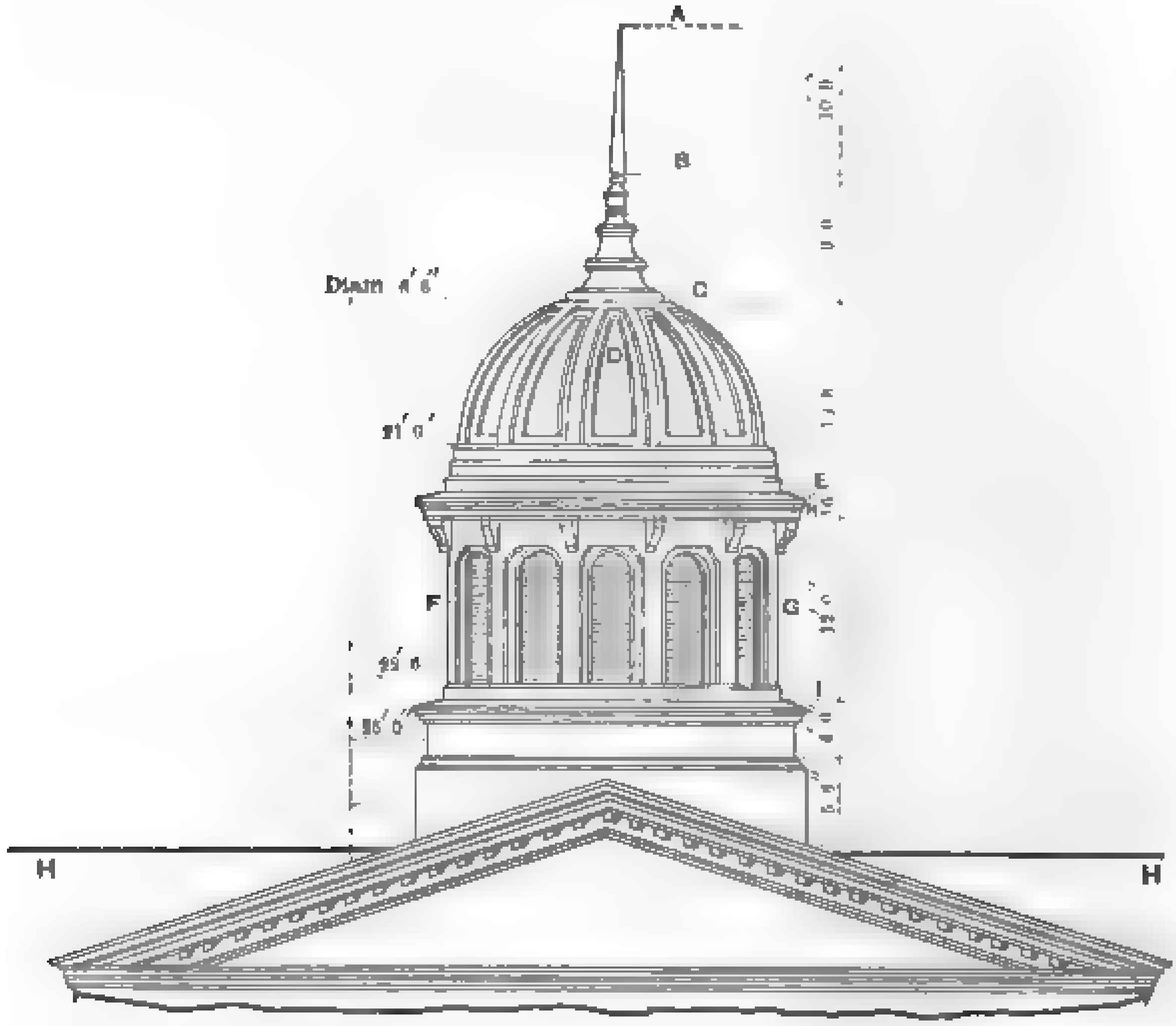
ART. XXII.—*Observations on the Light of Telescopes used as Night-Glasses*; by EDWARD S. HOLDEN.

IN the Philosophical Transactions for 1800, vol. xc, p. 67, Sir William Herschel says: "In the year 1776, when I had erected a telescope of 20 feet focal length, of the Newtonian construction, one of its effects by trial was that when toward evening, on account of darkness, the natural eye could not penetrate far into space, the telescope possessed that power sufficiently to show, by the dial of a distant church steeple, what o'clock it was, notwithstanding the naked eye could no longer see the steeple itself. Here I only speak of the penetrating power, for though it might require magnifying power to see the figures on the dial, it could require none to see the steeple."

I had long been desirous of trying this experiment with a large aperture, and made several attempts in 1874 to have the Dome of the 26 inch Clark refractor at Washington so arranged that a terrestrial object could be seen, but without success. I therefore took the first opportunity to try the effect of a telescope under these conditions at the Washburn Observatory, where the large equatorial commands the horizon. The most suitable object for examination was the tower of the Hospital

for the Insane, which is 20,798 feet distant from the center of the Dome.* 1″ at this distance is 1·3 inches; 1′ is 78 inches.

The accompanying figure will give the best idea of the object viewed. The drawing has been kindly made for me by W. V. Shipman, Esq., of Chicago. I have marked upon the cut the line of the horizon, from which it appears that the



whole tower has an elevation of about 9′ above the horizon line. In the observations which follow, the part A B, (10 feet high), is spoken of as "the spire;" B C, (9 feet), as "the base of the spire;" the next section, (13 feet high), as "the cupola" or "the dome," and the remaining portion, as "the tower."

The finder has an aperture of 3·50 inches, a field of 1° 20′, and a magnifying power of 26 diameters. The refractor has an aperture of 15·56 inches, a field of 11′·6, and a power of 195 diameters.

The following observations were made 1881, April 18, by Mr. S. W. Burnham and myself:

The whole sky was perfectly clear except a very faint bank of clouds to the west of the tower looked at. The observations were as follows: *Hn.* standing for observations made by Holden; β for those made by Burnham.

* I have to express my thanks to Professor J. E. Davies for the communication of the Coast Survey data from which these figures are derived.

7^h 35^m. The tower disappears to the naked eye. In the finder the spire is still plainly seen. In the 15-inch, the whole of the spire, ribs, dome and many details well seen.—*Hn.*
7^h 42^m. The tower disappears to the naked eye. In the finder and telescope everything still seen.—*β.*
8^h 0^m. 15-inch: the ribs on the cupola are gone.—*Hn.* and *β.*
8^h 7^m. Finder: the shape of the cupola is confused and uncertain.—*Hn.* and *β.*
8^h 14^m. Finder: pretty much the same. 15-inch: the spire on top of the cupola is still plain. No one looking with the telescope would miss it.—*Hn.* and *β.*
8^h 17^m. 15-inch: the spire on top of the cupola gone.—*Hn.* I still see it.—*β.*
8^h 17^m. Finder: all shape to the cupola is gone.—*Hn.*
8^h 21^m. 15-inch: spire still seen by averted vision; not well by direct.—*β.*
8^h 22^m. Finder: the tower is a mere black spot. 15-inch: spire is much fainter.—*β.*
8^h 23^m. 15-inch: the spire is gone, except that I can see that the outline of the cupola is not round.—*β.*
8^h 25^m. 15-inch: spire gone.—*β.*
8^h 26^m. Finder: tower gone.—*Hn.*
8^h 27^m. Finder: tower and cupola gone.—*β.*
8^h 27^m. 15-inch: tower and cupola gone.—*Hn.*
8^h 29^m. 15-inch: tower has lost all shape.—*β.*
8^h 30^m. 15-inch: tower gone.—*β.*

About this time the sky was dark and the horizon became clearer as was shown by small stars becoming visible in the finder. Probably the light cloud above spoken of was dissipated.

8^h 35^m. 15-inch: the cupola and tower can be plainly seen as a dusky cloud with a certain shape, when the telescope is vibrated to and fro.—*Hn.* and *β.*
8^h 37^m. 15-inch: same.—*Hn.* and *β.*
8^h 43^m. 15-inch: the cupola and tower are seen even better than before. The horizon is clearer. There is no difficulty in seeing them when the telescope is moved, and they can just be seen by direct vision.—*β.*
8^h 44^m. Same.—*Hn.* and *β.*
8^h 45^m. Stopped examination as there seemed to be no prospect of losing the tower as long as the horizon remained clear. If we had lost it we should have attributed the loss to haze at the horizon. Small stars 8-9 magnitude seen in finder. They must have had an altitude of not more than 30′.

It appears to me that this confirmation of Herschel's experiments is important, and worth the attention of physicists. So far as I know there is no satisfactory explanation of the action of the ordinary Night-glass, nor of the similar effect when large apertures are used.

Washburn Observatory, Madison, 1881, May 1.

ART. XXIII.—*On the nature of Dictyophyton*; by R. P. WHITFIELD. With a note, by J. W. DAWSON.

SINCE writing the article on *Dictyophyton* published in the last number of this Journal I have obtained additional evidence of their spongoid character. About the middle of May, while discussing their nature with Principal Dawson, of Montreal, we examined some allied forms from the Keokuk beds at Crawfordsville, Indiana, which lately came into the possession of the American Museum of Natural History, and found one which retained the substance of the organism. Under a hand-glass of moderate power it is seen to have been composed of cylindrical threads of various sizes, now replaced by pyrite. With the means then at our command it was impossible to fully determine whether they had been bundles of vegetable fibers or sponge-like spicules; but Dr. Dawson kindly offered to examine them more critically if I would forward a specimen to him at Montreal. This was done, and his note on their nature is appended below. The specimen used probably belongs to the genus *Uphantaenia* Vanuxem, and is a fragment about 2½ by 3 inches across and seems to have been a part of a circular or discoid frond of 8 or 10 inches diameter. It differs from *Uphantaenia Chemungensis* of New York in many features. The broad, radiating bands are more distant, with a narrow, thread-like band between; while all the circular bands have been narrow or thread-like. The spaces between the bands and threads are rectangular and covered by a thin film which is alternately elevated or depressed in the adjoining spaces, as if the bands had been elastic like rubber and had contracted, wrinkling up the intermediate spaces. A further description and illustration of the form I shall defer to a future occasion, but shall here designate the species as *Uphantaenia Dawsoni*. The broad bands are composed of very fine thread-like spicules, and the narrow ones of much stronger ones, while the thin film occupying the intermediate spaces is composed of still smaller spicules apparently arranged in radiating manner. The character and nature of these threads and spicules are well set forth in Dr. Dawson's notes below, and the spongoid features and relations to *Euplectella* indicated.

Note by Dr. J. W. DAWSON *on the Structure of a specimen of Uphantaenia, from the Collection of the American Museum of Natural History, New York City.*

To the naked eye the fossil presents rectangular meshes of dark matter on a gray finely arenaceous matrix. The spaces of the network are of an average size of 6^{mm} in length and 4 or 5

in breadth. The longitudinal bands are about 3^{mm} broad, the transverse bands much narrower. Some of the rectangular interspaces are of the color of the matrix; others wholly or partially stained with dark matter. The meshes are nearly black, but in a bright light show a fibrous texture and metallic luster due to pyrite.

Viewed as opaque objects under the microscope, the reticulating bands are seen to be fascicles of slender cylindrical rods or spicules, varying much in diameter; some of the largest being in the narrow transverse bands. The spicules may, in a few cases, be seen to be tapering very gently to a point, but usually seem quite cylindrical and smooth. In their present state they appear as solid shining rods of pyrite. The largest spicules are about $\frac{1}{500}$ of an inch in diameter; the smaller scarcely one-fourth of that size. The spicules of the transverse bands cross those of the longitudinal ones without any organic connection. Among the long spicules of the bands can be seen multitudes of very minute and apparently short spicules confusedly disposed, and these abound also in the dark-colored areoles.

On the whole the structures are not identical with those of any plant known to me, and rather resemble those of siliceous sponges of the genus *Euplectella.*

The most puzzling fact in connection with this view is the mineral condition of the spicules now wholly replaced by pyrite. Carbonaceous structures are often replaced in this way and so are also calcareous shells, especially when they contain much corneous matter, but such changes are not usual with siliceous organisms. If the spicules were originally siliceous, either they must have had large internal cavities which have been filled with pyrite, or the original material must have been wholly dissolved out and its place occupied with pyrite. It is to be observed, however, that in fossil sponges the siliceous matter has not infrequently been dissolved out, and its space left vacant or filled with other matters. I have specimens of *Actylospongia* from the Niagara formation which have thus been replaced by matter of a ferruginous color; and in a bundle of fibers probably of a sponge allied to *Hyalonema* from the Upper Llandeilo of Scotland, I find the substance of the spicules entirely gone and the spaces formerly occupied by them empty. It should be added that joints of Crinoid stems and fronds of *Fenestella* occurring in the same specimen with the *Uphantaenia* are apparently in their natural calcareous state.

Though I have hitherto regarded this curious organism as a fucoid, I confess that the study of the specimen above referred to inclines me to regard it as more probably a sponge.

I owe the opportunity of examining this very interesting specimen to the kindness of Professor Whitfield.

ART. XXIV.—*Note on Photographs of the Spectrum of the Comet of June*, 1881; by Professor HENRY DRAPER, M.D.

THE appearance of a large comet has afforded an opportunity of adding to our knowledge of these bodies by applying to it a new means of research. Owing to the recent progress in photography, it was to be hoped that photographs of the comet and even of its spectrum might be obtained and peculiarities invisible to the eye detected. For such experiments my observatory was prepared, because for many years its resources had been directed to the more delicate branches of celestial photography and spectroscopy, such as photography of stellar spectra and of the nebulæ. More than a hundred photographs of spectra of stars have been taken, and in the nebula of Orion details equal in faintness to stars of the 14·7 magnitude have been photographed.

It was obvious that if the comet could be photographed by less than an hour's exposure, there would be a chance of obtaining a photograph of the spectrum of the coma, especially as it was probable that its ultra-violet region consisted of but few lines. In examining my photographs of the spectrum of the voltaic arc, a strong band or group of lines was found above H, and on the hypothesis that the incandescent vapor of a carbon compound exists in comets this band might be photographed in their spectrum.

Accordingly, at the first attempt, a photograph of the nucleus and part of the envelopes was obtained in seventeen minutes on the night of June 24th, through breaks in the clouds. On succeeding occasions, when an exposure of 162 minutes was given, the tail impressed itself to an extent of nearly ten degrees in length.

I next tried by interposing a direct vision prism between the sensitive plate and object glass to secure a photograph which would show the continuous spectrum of the nucleus and the banded spectrum of the coma. After an exposure of eighty-three minutes, a strong picture of the spectrum of the nucleus, coma and part of the tail was obtained, but the banded spectrum was overpowered by the continuous spectrum.

I then applied the two-prism spectroscope used for stellar spectrum photography, anticipating that although the diminution of light would be serious after passing through the slit, two prisms and two object glasses, yet the advantage of being able to have a juxtaposed comparison spectrum would make the attempt desirable, and moreover, the continuous spectrum being more weakened than the banded by the increased dispersion the latter would become more distinct.

Three photographs of the comet's spectrum have been taken with this arrangement with exposures of 180 minutes, 196 minutes and 228 minutes, and with a comparison spectrum on each. The continuous spectrum of the nucleus was plainly seen while the photography was in progress. It will take some time to reduce and discuss these photographs and prepare the auxiliary photographs which will be necessary for their interpretation. For the present it will suffice to say that the most striking feature is a heavy band above H which is divisible into lines, and in addition two faint bands, one between G and *h* and another between *h* and H. I was very careful to stop these exposures before dawn, fearing that the spectrum of daylight might become superposed on the cometary spectrum.

It would seem that these photographs strengthen the hypothesis of the presence of carbon in comets; but a series of comparisons will be necessary, and it is not improbable that a part of the spectrum may be due to other elements.

271 Madison Avenue, New York.

ART. XXV.—*Spectroscopic Observations upon the Comet b*, 1881; by Professor C. A. YOUNG.

WHILE the Comet was brightest the weather at Princeton was very tantalizing. From June 25 to July 3, the comet was seen and observed on every night except June 30; and on none of them, except July 2, more than an hour at a time, the work being invariably interrupted by clouds or fog.

For the spectroscopic observations I have used both the one-prism instrument, by the Clarks, which belongs with the Equatorial, and the solar spectroscope by Grubb—the latter with dispersive powers varying, according to occasion, from two to six dense glass prisms. The telescope was the 9½ inch Equatorial.

The following are the principal facts made out so far:

(1.) The spectrum of the nucleus was found to be for the most part simply continuous; but on several occasions, especially June 25, July 1, and July 12, it showed distinct bands, coinciding with those of the spectrum of the coma. When brightest the spectrum could easily be followed from the neighborhood of B to a point well above G; and in the lower portion it showed color strongly.

(2.) The spectrum of one of the jets which issue from the nucleus was isolated on June 29th and found to be continuous. I think this was usually the case with the jets, but it is seldom possible to separate the spectrum of a jet from that of the nucleus sufficiently to be perfectly sure.

(3.) The spectrum of the tail appears to be a continuous spectrum overlaid by a banded spectrum, the same as that of the coma. The bands in the spectrum of the tail were followed to a distance of about 20′ from the head, on June 29 and July 1. The continuous spectrum ceased to be visible before the bands were entirely lost sight of, using a slit wide enough to unite the *b's* into one band.

(4.) The spectrum of the coma shows only three bright bands with a faint continuous spectrum connecting them. No other bands could be found, though the continuous spectrum could be followed from about half way between C and D, to above G. The Fraunhofer lines could not be seen either in the spectra of the nucleus or of the coma.

While the comet was brightest, the bands, especially the upper and lower ones, were very ill-defined, so much so as to interfere with satisfactory measurements of position. After July 1 the definition became better.

(5.) The coma spectrum was very carefully compared with the spectrum of the Bunsen burner flame, with the spectra of Geissler tubes containing CO, CO_2 and ether vapor, and also with the spark spectrum of magnesium and air. The wave length of the less refrangible edges of each of the three bands was carefully determined by micrometer measures, on June 29, and on July 1, 2, 3, 6 and 12.

All the comparisons concur in showing a close, and so far as the dispersive power employed could decide, an *exact* agreement between the spectrum of the comet and that of the Bunsen flame. On the other hand the discordance between the comet-spectrum and the spectra of the Geissler tubes was striking. The lower of the three comet bands was the only one which was even approximately coincident with any band of the tube spectrum.

(6.) The measurements on the evenings named give the following numbers for the wave-lengths of the bands, viz:

Lower edge of lower band, $\lambda = 5629\cdot \pm 4\cdot0$
Lower edge of middle band, $\lambda = 5164\cdot9 \pm 0\cdot6$
Lower edge of upper band, $\lambda = 4740\cdot \pm 2\cdot9$

The lower band was much the most difficult to deal with. The maximum of brightness seems to be, not at the edge of the band, but a little way up, and this perhaps may explain the fact that I obtained 5564 in the case of Hartwig's comet (while Von Konkoly obtained 5610—a much better result). Dr. Watts (Nature, vol. xx, page 28) gives 5634·7, 5165·3 and 4739·8 as the wave-lengths for the corresponding bands in the spectrum of the Bunsen flame.

(7.) The middle band, on June 29, July 1, 2, and 3, showed

three fine, bright lines upon it, one just at the lower edge of the band, and the other two at distances of about 30 Ångstrom units—coinciding apparently with three lines which are seen in the Bunsen flame spectrum, though I did not succeed in measuring them.

It is hardly necessary to say that the evidence as to the identity of the flame and comet spectra is almost overwhelming; the peculiar ill-defined appearance of the cometary bands at the time of the comet's greatest brightness is, however, something which I have not yet succeeded in imitating with the flame spectrum. The comet spectrum on July 25th certainly presented a general appearance quite different from that of the later observations, as regards the definition of the bands.

Perhaps I may be allowed to record here a fact which has nothing to do with the comet, but was observed while adjusting the spectroscopes upon the sun in preparation for evening work. I find that the one-prism spectroscope shows the bright lines in the upper portion of the chromosphere spectrum, above *h*, better than any other instrument I have yet tried. I have hitherto always found it rather difficult to exhibit the two H's as bright lines to a person unused to the spectroscope, but with this instrument they are perfectly obvious—even obtrusive. The only (and indispensable) precaution needed is to put the slit accurately in the focal plane of the telescope for these special rays.

Princeton, July 14.

ART. XXVI.—*Note on the Observations of Comet b*, 1881, *made at the United States Naval Observatory;* by WM. HARKNESS.

[Communicated by authority of Rear Admiral John Rodgers, U. S. N., Superintendent.]

ON the evening of June 28th, I examined the comet for polarization by means of a double image prism applied to the naked eye, and at first I fancied that when the two images were placed in the axis of the tail the one situated farthest forward was the fainter, but a careful examination by three different observers rendered this doubtful. Recourse was then had to a three-inch telescope armed with an eye-piece magnifying 34·5 diameters, and the image of the comet given by it was examined, first with the double image prism, and subsequently with a Savart polariscope, but neither of these instruments showed any polarization. Mr. Huggins thinks he has detected the

Fraunhofer lines in the continuous spectrum of the nucleus, and if this really is the case its light must be at least partly derived from the sun, and should show traces of polarization. As just stated, I failed to discern any with the double image prism; but that is not a very delicate test, although, owing to the small size of the nucleus, it is almost the only one practicable. Under the magnifying power used the coma filled the field of view with bright light, and yet exhibited not a trace of polarization when tried by that most delicate of all tests, the Savart polariscope; thus apparently confirming the testimony of the spectroscope that the coma is self-luminous.

On the evenings of June 28th, and July 1st and 2d, I examined the spectrum of the comet with a spectroscope having a single sixty-degree prism through which a beam of light 0·82 of an inch in diameter is passed. The wave-lengths of the bands in the comet's spectrum were determined by measuring the interval between them and the D line given by the flame of a spirit lamp with a salted wick held before the object glass of the telescope to which the spectroscope was attached; the measurement being effected by a micrometer which showed a bright point in the field of view. Owing to the unfavorable position of the comet, the only telescope upon which the spectroscope could be used was my three inch of 43·6 inches focus, which is mounted upon a portable tripod stand, but is destitute of clamp and tangent screws.

Notwithstanding the brightness of the comet, it gave a spectrum very ill-defined, and difficult to measure. The spectrum of the nucleus seemed to be continuous, and its approximate extent was from D to G. I did not detect any Fraunhofer lines in it, but possibly they may exist and yet have been obliterated by the rather wide opening of the slit, which was 0·0125 of an inch. With a narrower slit it was difficult to keep the comet in the field of the spectroscope. The coma gave a spectrum consisting of three bright bands, so ill-defined that no precise measures of the wave-lengths of their edges could be made, but the wave-lengths of their brightest parts were respectively, 549·3, 512·4 and 467·2. This seems to be the ordinary comet spectrum. The measurement of the wave-length of the middle band is tolerably accurate, but the measurements of the other two are liable to considerable uncertainty, owing to the faintness of the bands. I estimated their relative brightness to be 5, 30 and 1. On July 1st a slight haziness of the atmosphere sufficed to render the third band invisible. At a short distance from the head of the comet this band always faded out, and the spectrum of the tail seemed to consist of the first and second bands only—that is 549·3 and 512·4.

On June 28th the comet's nucleus was about as bright as a third-magnitude star, and its tail was plainly visible throughout an extent of at least twelve degrees. On July 1st the comet was perceptibly fainter, and its tail was only about eight degrees long, but perhaps this was partly owing to the moon, five and a half days old, being above the horizon. On July 2d the atmosphere was very clear and the seeing good, but the visibility of the comet was much diminished by the brightness of the moon, then near its first quarter. I estimated the length of the tail to be about the same as on the preceding evening, but Mr. Rock thought he could trace it for rather more than twenty degrees.

Since the 10th inst., Professor Hall has examined the comet with the twenty-six inch refractor, and Professor Eastman has examined it with the nine and six-tenth inch refractor, but neither of these gentlemen have been able to see any indications of a division of the nucleus.

The comet was observed at its lower culmination, with the transit circle, on June 26, 27, 28, 29 and July 1, 2, 3, 5, 6, 10, 11. For the convenience of those who may desire to compute the orbit, Professor Eastman has furnished from these observations, the following positions, which are uncorrected for parallax and aberration time:

Washington Date.	Right Ascension.	Declination.
June 26·5	$5^h\ 48^m\ 38^s{\cdot}04$	$+57°\ 40'\ 52''{\cdot}0$
July 1·5	6 22 46 ·85	70 39 57 ·6
" 3·5	6 42 32 ·92	74 5 16 ·2
" 6·5	7 20 36 ·88	77 49 56 ·3
" 10·5	8 27 31 ·84	+80 48 56 ·1

From a Cambridge observation of June 23d, and the Washington observations of June 29th and July 5th, Professor Frisby has computed the following parabolic elements:

$$\text{Perihelion Time, June } 16{\cdot}3700$$
$$\left.\begin{array}{rl} \pi = & 265°\ 31'\ 15''{\cdot}4 \\ \Omega = & 270\ \ 58\ \ 27\ {\cdot}0 \\ i = & 63°\ 25'\ 55''{\cdot}7 \\ \log q = & 9{\cdot}866748 \end{array}\right\}\ \begin{array}{c}\text{Equinox}\\ 1881{\cdot}0\end{array}$$

The residuals, C—O, for the middle place are

$$\delta\lambda \cos\beta = -\ 13''{\cdot}4$$
$$\delta\beta = +\ 62\ {\cdot}1$$

It is a matter of interest to note that about June 20th the earth was in the immediate vicinity of the comet's tail, but I have not made sufficiently accurate computations to be able to state whether or not it actually passed through it.

U. S. Naval Observatory, Washington, July 13, 1881.

ART. XXVII.—*Observations on the Comet* 1881 *b;* by LEWIS BOSS.

NEWS of the sudden appearance of a great comet in the northern sky first reached me through the local newspapers on June 23; but that night was cloudy. On the evening of June 24, the comet was occasionally seen for a few moments at a time, through intervals in clouds, but never with sufficient clearness to admit of satisfactory examinations as to its physical appearance. One micrometric comparison between the comet and DM 50° 1225 was secured with the thirteen-inch refractor. The comet was plainly visible to unassisted vision in a clear sky at sixteen hours mean time, and then appeared as bright as Capella.

Owing to an accident which happened to the equatorial during my absence, I have thus far been unable to secure additional micrometric comparison by that instrument. At lower culmination the comet has usually been hidden by clouds, and the hour is now very inconvenient; so that I can report only the following observations of apparent position:

D. O. M. T.	App. α.	App. δ.	
June 24^d 9^h 59^m 31^s	5^h 39^m 14^s·2	+ 49° 59′ 20″·	Filar micrometer.*
June 26 11 26 51	5 48 35·53	+ 57 39 05·2	Transit circle.
June 28 11 30 26	6 00 00·69	+ 63 43 31·8	Transit circle.
July 8 12 42 38	7 51 49·54	+ 79 34 03·0	Transit circle.

From the first three positions reduced to 1881·0 and corrected for parallax and aberration by means of values of Δ from a preliminary orbit, I derived the following parabolic elements.

$$T = 1881, \text{ June } 16{\cdot}1358. \quad \text{Washington M. T.}$$

$$\left.\begin{array}{lrrr} \pi & 265° & 01' & 38'' \\ \Omega & 270 & 58 & 45 \\ i & 63 & 30 & 27 \end{array}\right\} 1881{\cdot}0$$

$$\log q \;\; 9{\cdot}86510$$

Middle place, C—O. $\Delta\lambda \cos\beta$, $+4''$. $\Delta\beta$, $-7''$. We also have with the same elements: July 8, C—O. $\Delta\lambda \cos\beta$, $+30''$. $\Delta\beta$, $-75''$. The elements therefore are not likely to be found greatly in error.

The similarity of the elements of this comet with those deduced by various computers for the comet of 1807, has already been much discussed in the newspapers. The difference of about three degrees in the position of the nodes, and especially the great difference in the respective values of q (which amounts to ·087) seems larger than can well be ascribed to errors of computation, or possible planetary disturbance.

* Star of comparison DM 50° 1225, position obtained from Argelander's northern zones combined with Bonn VI. $\Delta\alpha$ on three wires; $\Delta\delta$, one measure.

It seems to me more likely that these two comets may have formed parts of the same body in distant ages, and that these parts may have separated as Biela's comet did. The two parts would need to have but slightly differing mean distances from the sun in order eventually to reach the amount of separation which now exists between the perihelion passages of the 1807 and 1881 comets. A great number of similar, though generally less striking resemblances among cometic orbits have been noted, in cases where absolute identity between the two comets considered seems impossible. These cases increase the demand for a general explanation, such as I have suggested above. The resemblances seem to be too close and too frequent to be considered the result of chance; and the above hypothesis seems to have some support in reason and experience. If the comet of 1881 proves to have a periodic time between one and two thousand years the plausibility of this hypothesis will be very much strengthened.

I have been too much pressed with other duties to give close or systematic attention to the physical characteristics of the comet. The nights of June 26, June 28, July 1, 8 and 13 were unusually favorable for such studies here. The atmosphere was unusually transparent on June 26 and I then traced the tail for a distance of nearly forty degrees from the nucleus. On that night there were two branches. The longer and brighter branch was perfectly straight. The other curved, with its concavity toward greater right ascension. On the next clear night (June 28) the straight branch was of about the same length as the curved one, and was a thin and scarcely perceptible streak. On July 1, the two branches seem to have merged into one, presenting a shorter and broad fan-like appendage, perfectly straight and strongly marked on the preceding side, concave and nebulous on the following.

On all occasions the nucleus under a power of 250 has seemed to be quite distinctly defined and star-like in appearance. On June 26, its measured diameter was $7''$; on July 8, this had become $2''$. The latter measure reduced to the distance of June 26 becomes $3''\cdot3$, a rather surprising reduction in the diameter, if it be real.

Dudley Observatory, Albany, N. Y., July 19, 1881.

ART. XXVIII.—*The Polarization of Light from Comet b*, 1881; by ARTHUR W. WRIGHT.

POLARISCOPIC observations of the comet were made on the evenings of June 25 and 26, which gave faint indications of the existence of polarization, but with the instruments then used it was not possible to ascertain satisfactorily either its character or amount. The state of the sky was not very favorable for observation until the evening of June 29, when a method of observation was found which made it possible to determine the polarization, which was at once seen to be considerable, with comparative ease and a good degree of precision.

The instrument employed was the polarimeter constructed for observation of the solar corona in the eclipse of July 29, 1878, and described in the volume containing the reports upon this eclipse issued from the U. S. Naval Observatory.* A slight modification was made by substituting a Savart plate for the selenite, it being attached to the Nicol's prism in the eye-piece. This gave a rather narrow field which was nearly filled by the image of the comet, an arrangement very favorable for detection of the bands caused by polarization. The aperture of the telescope to which the polarimeter is attached is three inches.

The plane of polarization of the light was found to have such a direction as to pass through the sun's place. This was determined both by the disposition of the bands seen in the polarimeter, and also independently by means of a double-image prism placed before the ordinary eye-piece of the telescope when this was attached to the instrument. The two images of the comet as the prism was rotated were easily seen to have different intensities in certain positions corresponding to polarization in a plane situated as above described. As seen with this instrument the fainter of the images appeared considerably shorter than the other as if the light coming from toward the extremity of the tail were more strongly polarized than that from points near the nucleus. But this was possibly an illusion depending upon the fact that when the very faint light was diminished by the polarizing effect it became too feeble for perception, and this lessened the extent of the visible area. When examined with the polarimeter the light appeared to be slightly less strongly polarized as the instrument was directed to points more remote from the head of the comet, as would be the case if the proportion depended simply upon the angle of incidence of the light, which decreased with the distance from

* Reports on the Total Solar Eclipses of July 29, 1878, and January 11, 1880, pp. 264–267.

the nucleus. The second and third of the observations of July 1, in the list below, were made upon regions removed several degrees from the nucleus, but though the amount of polarization is somewhat less, and tends to confirm the above conclusion, the difference is hardly greater than would be accounted for by the errors of observation. Determinations of polarization at great distances from the nucleus were not possible, the light being too feeble.

In the use of the polarimeter, the latter was fitted so that a card could be attached to the slide moving the glass plates. The positions were pricked upon this with a needle point, and were read off by means of the graduated circle after the observations were finished. The latter were made in sets of ten, the plates being moved to the point of neutralization, or disappearance of the bands, first from below, then from above, alternately, until five had been made from each direction. The points upon the card thus fell into two groups separated by an interval which was greater or less according to the degree of polarization. The mean of the angles for each set of five being taken, the percentage of polarized light corresponding to each was determined from a curve constructed for the instrument. Two values were thus obtained the mean of which was the amount of polarization for the point observed. Each card was capable of containing two sets of points, and could be removed or replaced by another without the aid of a light, a necessary precaution in observations of such delicacy, as the proper sensitiveness of the eye could only be maintained by seclusion from the light.

The results of the observation are given in the following table. The date and local mean time for the series of each evening are given in column I. In column II are given the results derived from the sets of determinations arranged in their order as made, each result, as explained above, being obtained by ten measurements. The numbers express the proportion of polarized light to the total light reckoned as one hundred parts. The means of the percentages of the sets in column II for each evening are given in column III. Column IV gives the approximate angles of incidence of the light derived from the sun, referred to the nucleus or points very near it. It is obtained from the ephemeris of Peters,* combined with that of Oppenheim given in the Dun Echt Circular No. 24. The angles subtended at the comet by the earth's radius vector at the dates of the ephemeris were obtained by a simple graphic process. With these a curve was constructed from which the angles for the dates of the different observations were derived. These divided by two are the angles of incidence.

* Astronomische Nachrichten, No. 2381, p. 75.

I.	II.	III.	IV.
June 29, 1^h to 2^h, A. M.	24·7 23·8 21·3	23·3	60°·5
June 30, 1^h to 2^h, A. M.	18·1 17·6 18·6 17·1	17·8	58°
July 1, 11^h to 12^h 30^m P. M.	21·8 20·1 17·6 17·7	19·3	54°·5
July 2, 10^h 30^m to 11^h P. M.	16·9 16·9	16·9	52°·5
July 3, 10^h 30^m to 12^h P. M.	18·3 18·0 18·7	18·3	51°
July 21, 11^h 30^m P. M. to 1^h.	15·9 15·6	15·7	33°
July 22, 11^h 30^m P. M. to 1^h.	14·5 13·8	14·1	32°

The observation of July 2 was made under rather unfavorable atmospheric conditions, and the sky was somewhat luminous from auroral action. The amount of polarization found is undoubtedly less than the true value. The others were made when the sky was very clear, and during those of July 21 and 22 it was exceptionally fine. The time of the observations precluded the possibility of any influence from twilight or the light of the moon.

On comparing the percentage of light polarized and the angles of incidence it is seen that they decrease together. No definite maximum was made out, but the existence of one near or beyond 60° is perhaps indicated by the fact that polarization was less easily observed on the evenings previous to June 29, and by the more rapid variation in the percentages on this and the two succeeding evenings. At first sight the large percentages obtained in the earlier observations appeared to indicate reflection from a gaseous substance, but the numbers found later, and especially the relation of all the values to the angles of incidence, render an inference as to the character of the reflecting material more difficult. It is not improbable that the constitution and physical condition of the matter composing the tail were variable, and this circumstance would introduce changes in the proportion of polarized light, in addition to those produced by the alteration in the angle of the reflected rays. The fact of polarization shows that a large part, probably the greater part, of the light coming from the tail is reflected sunlight.

Yale College, July 25, 1881.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On Ozone as a cause of the Luminosity of Phosphorus.*—Various writers, especially Joubert, have called attention to the connection of the phenomena of phosphorescence with ozone. To learn something of the nature of this connection, Chappuis has studied the effect of ozone upon the luminosity of phosphorus in the presence of oxygen. Fourcroy had long ago observed that in pure oxygen at a temperature of 15° and under atmospheric pressure, phosphorus is not luminous in the dark. Chappuis now finds that under these conditions a bubble of ozone introduced into the bell jar produces the phosphorescence, though only momentarily, the ozone being destroyed. Moreover, it is not the vaporization of the phosphorus which determines the phosphorescence, but the combustion of this vapor, the entire space occupied by the oxygen at first appearing luminous, the solid becoming so only after all the vapor has been burned by the ozone. Two cylinders, one containing air, the other pure oxygen, were inverted over two dishes containing iodide of potassium and starch solution. A fragment of phosphorus was plunged into each gas, in contact with the liquid. In the first, the phosphorus became luminous and the solution became blue. In the second neither phenomena appeared. Whenever the phosphorescence appeared, ozone was present; and whenever ozone was absent there was no luminosity. Moreover, the author calls attention to the fact that certain bodies which have the power of preventing this luminosity of phosphorus are precisely those bodies which destroy ozone or are destroyed by it. Oil of turpentine for example, which is the most active, destroys ozone completely. In a balloon containing air, phosphorus and turpentine, a bubble of ozone produces light for a second only, the ozone being destroyed by the turpentine, but burning a part of the phosphorus vapor also. On adding the ozone the luminosity extends throughout the space and at last the solid phosphorus only remains luminous. Hence the author regards the production of the luminosity of phosphorus in oxygen, as one of the most delicate of the reactions for ozone and proposes to employ it in subsequent researches.—*Bull. Soc. Ch.*, II, xxxv, 419, April, 1881.

G. F. B.

2. *On the appearance of Nitrous Acid during the Evaporation of Water.*—Warington has submitted to the test of careful experiment Schönbein's statement that whenever pure water or an alkaline solution is evaporated, nitrite of ammonium is produced, and concludes that "it is undeniable that pure water if evaporated to a small bulk, by ordinary means, will generally be found to contain nitrous acid." A sample of rain water which gave no reaction to the metaphenylenediamine test, after concentration to one quarter of its bulk, showed the reaction distinctly. A liter of distilled water, with 5cc. lime water, evaporated to a small volume

over a Bunsen burner gave a strong reaction. The importance of this result, with reference to the determination of nitrites in natural waters, led to an investigation of the cause of this result. First it appeared that a liter of distilled water evaporated in a retort, either exhausted or at atmospheric pressure, gave no reaction for nitrous acid, and hence proved the air to be the source of the contamination. Another liter with 5cc. lime water, was evaporated in a glass basin 6¼ inches in diameter over a Bunsen rose burner. The reaction given was strong and corresponded to about 0·05 mgrm. of nitrogen. Since a second similar evaporation conducted with steam gave only 0·004 mgrm. nitrogen, it was clear that the nitrous acid had mainly come from the combustion of the gas used as fuel. But still even in the residue obtained with steam, the rose-color appeared. That this came directly from the air of the room was shown by placing a second basin of distilled water by the side of the first during the evaporation. After twenty-four hours a full rose tint was developed; and this without any sensible evaporation. For extremely accurate work, water then must be evaporated in close vessels; but for ordinary purposes, the concentration may be effected in a steam bath.

Warington gives in his paper some experiments made with the naphthylamine test for nitrous acid, proposed by Griess, which show an extraordinary delicacy. The solution to be tested was slightly acidified with hydrochloric acid and a few drops of an aqueous solution of sulphanilic acid and of a similar solution of naphthylamine hydrochloride were added. The nitrous acid if present forms a diazo-compound, which the naphthylamine converts into a body having a beautiful rose color. The tests for delicacy were made in test tubes, the column of liquid being about three inches deep. To 10cc. of the solution were added one drop of HCl (1 : 4) one drop of a nearly saturated solution of sulphanilic acid and one drop of a saturated solution of naphthylamine hydrochloride. The standard solution was made with potassium nitrite prepared from pure silver nitrite. With a solution of 1 part of nitrogen as nitrous acid in 1,000,000 parts of water an immediate pink color appeared which rapidly became deep ruby red; in 10,000,000 parts, at once a pink, and at the end of an hour a full rose; in 100,000,000 parts a pink tinge in six minutes and a pale pink in an hour; in 500,000,000 parts (1cc. of the millionth solution in a half liter of water) showed a pink tinge before two hours, and in twenty-four hours the three inch column showed it; 1,000,000,000 parts using ten drops of the reagents, showed an alteration of tint in two hours, and a distinct pink color in twenty-four hours. In the last two experiments similar flasks to which no nitrite had been added were similarly treated, but without result. During the reading of the paper in the Chemical Society's room, the presence of nitrous acid in the air was shown by exposing 200cc. of water containing the test, to the atmosphere there in a basin for four hours. On pouring it into a cylinder, it had become rose-pink as was seen on comparing it with a similar cylinder which had been closed with a

watch glass. In the open air at the Rothamsted farm, nitrous acid was detected by this air test. In six days the reaction appeared in water exposed to this air, and in twenty-seven days it contained one part of nitrogen in 15,000,000. In rain water the naphthylamine test readily shows nitrous acid, except when the rains are exceptionally heavy.—*J. Chem. Soc.*, xxxix, 229, May, 1881. G. F. B.

3. *On Boron hydride.*—JONES and TAYLOR have examined with care the preparation and properties of the boron hydride discovered by the former in 1879. Three methods of preparing magnesium boride were used: 1st, the action of recently ignited boric oxide, finely powdered, upon magnesium dust; 2d, the direct union of magnesium and boron; and 3d, the action of magnesium on boron trichloride. Though the two latter methods yielded a purer product, the first was the more convenient. The magnesium boride is placed in a flask with a little water and strong hydrochloric acid is added. The evolved gas may be collected over water or mercury. It is boron hydride mixed with a large quantity of hydrogen. In this condition it is a colorless gas which has an extremely disagreeable and characteristic odor, producing nausea and headache, is slightly soluble in water, which does not decompose it, burns with a splendid green flame, producing boric oxide, is decomposed by passage through a red-hot tube, depositing a brown film of boron, and depositing boron on a porcelain plate held in its flame, is decomposed when passed through a solution of silver nitrate giving a black precipitate containing boron and silver, and is oxidized to boric acid by potassium permanganate solution. With ammonia it gave a compound decomposed by acids. On analysis the boron appeared to be combined with 2·86 parts H; confirming the formula BH_3.—*J. Chem. Soc.*, xxxix, 213, May, 1881. G. F. B.

4. *On the Purification of Carbon Disulphide.*—ALLARY has proposed a simple, rapid and effective method of purifying carbon disulphide. This consists in covering it with a layer of water to which, from time to time, portions of a concentrated solution of potassium permanganate are added. The whole is strongly agitated after each addition, the process being stopped when the reduction of the permanganate is no longer produced, and the water retains its purple color. After washing several times, the disulphide is obtained free from water by means of a separating funnel, and filtered through a thick dry paper. Redistillation is seldom necessary. The odor is etherial and not at all disagreeable. It should be kept in the dark.—*Bull. Soc. Ch.*, II, xxxv, 491, May, 1881. G. F. B.

5. *Electric Absorption of Crystals.*—Professor H. A. Rowland and Mr. E. H. Nichols discuss the question whether there should be electric absorption in a perfectly homogeneous medium. The theory indicates that there should be none, and the writers have tested the point by experiment, and it was found that Iceland spar had no electric absorption. This substance can be

regarded as perfectly homogeneous. The writers consider that the apparatus which they used will be of value in testing the perfect homogeneity of insulating bodies.—*Phil. Mag.*, June, 1881, p. 414. J. T.

6. *Transmission of radiation of low refrangibility through Ebonite.*—Captain Abney and Colonel Festing have repeated the experiments of Professor Bell which showed that invisible rays of heat, of low refrangibility, pass through ebonite, by exposing a sensitive photographic plate to these radiations. An image was formed in many cases and the writers conclude that the coëfficient of absorption of a plate of ebonite $\frac{1}{64}$ of an inch in thickness is equal to 1·8 and that any rays which can penetrate through $\frac{1}{8}$ of an inch of ebonite will only have an intensity of $\frac{1}{1650000}$ of that of the resultant beam without deducting anything for the scattering of the light. It is concluded "that ebonite when of small thickness transmits to some extent the rays of low refrangibility."—*Phil. Mag.*, June, 1881, p. 466. J. T.

7. *Conservation of Electricity.*—M. G. Lippmann continues his paper on this subject—see *Comptes Rendus*, May 2, 1881—and maintains that the principle of the conservation of electricity stands in the same relation to electricity that Carnot's principle stands to heat.—*Comptes Rendus*, p. 1149, No. 20, May 16, 1881. J. T.

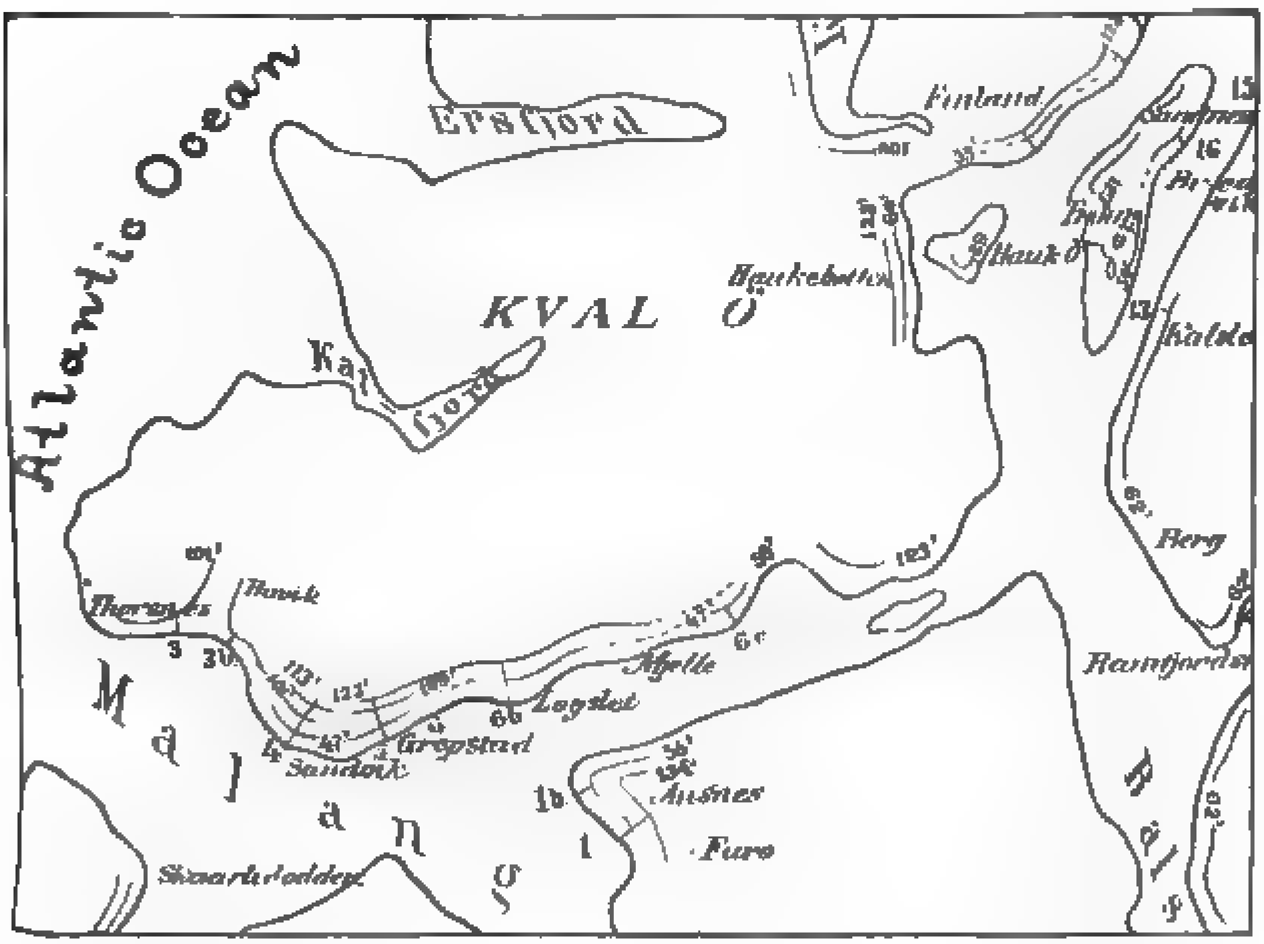
8. *Heating of Ice.*—A. Wüllner repeats the experiments of Carnelley and concludes that so long as the bulb of the thermometer is wholly surrounded with ice the thermometer indicates no temperature above —3° C. The thermometer with its bulb encased in ice was placed in an air-tight test tube, which was enlarged by a connecting tube of glass ending in a larger receptacle; the air contained in this could be raised or lowered in temperature and thus the temperature of the air in the test tube could be modified. When this air was heated by a Bunsen burner the thermometer rose quickly to −3° C. The ice vaporized quickly; when the bulb of the thermometer ceased to be completely surrounded by ice the temperature rose to 0° C., and when the thermometer bulb became more free from ice the temperature rose very quickly as the ice vaporized. Wüllner confirms the observation of Carnelley that the thermometer under these conditions could rise from 20° C. to 30° C. above zero and pieces of ice still be observed hanging to the thermometer bulb.—*Wied. Annalen der Physik und Chemie*, No. 5, 1881, p. 105. J. T.

9. *Atomic Weight of Cadmium.*—Mr. Oliver W. Huntington, of Harvard, has made a study of the atomic weight of cadmium, following the method used by Prof. Cooke with reference to antimony. The mean result from his first series of experiment is 112·31; and from a second, 112·32.

II. Geology and Mineralogy.

1. *Terraces and ancient Coast lines* (" *Strandlinien*") ; by Karl Pettersen. Published in Norwegian, at Tromsö, in 1880, and translated into German by Dr. R. Lehmann, Zeitsch. f. d. gesammt. Naturwissenschaften, liii, 1880.—Prof. Pettersen has carried on an extended series of observations of the system of terraces in northern Norway. The region examined extends from north to south about 50 English miles and an equal distance from east to west, embracing the fiords and sounds in the neighborhood of Tromsö (lat. 70° N.). A portion (about one-fourth) of the map accompanying his article is reproduced in fig. 1, of the same scale as the original. The terrace system includes first the proper terraces of loose material, sand, gravel and so on, and secondly the "strandlinien" or coast lines which are worn out of the solid

1.

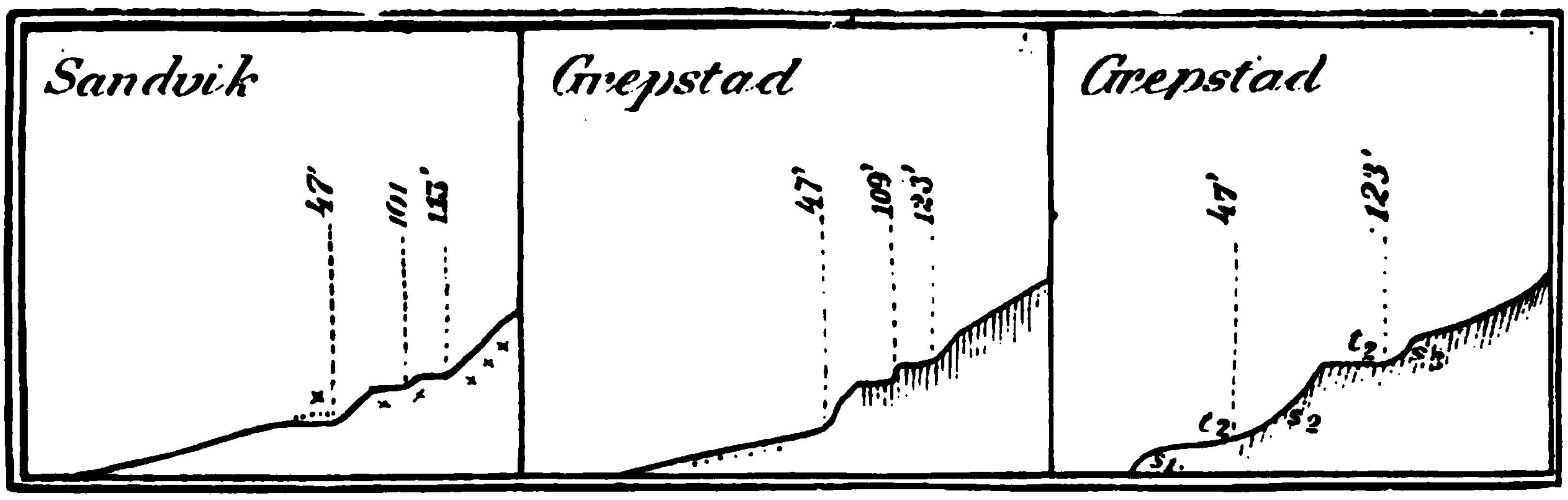


rock. Each of them consists of two parts, the more or less level upper surface, and the slope which bounds it in front (see t_2 and s_1, s_2, s_3 in fig. 4). A survey of all the results of observation shows that these bench lines occur at almost every height from the lower limit up. Between the bench lines at 13·9 and 42·7 meters above the sea the average difference in height of any two successive lines is only 2·2 meters, while the maximum is never greater than 3·8 ; above the upper limit named the same may be true but the number of lines observed is smaller.

Figure 1 represents these several lines at the different points on

the coast near Tromsö, and figs. 2, 3, 4 give sections at three points (see also fig. 1), which may fairly be taken as typical. Figure 2 is from Sandvik, where are three levels, namely, 14·5, 31·6 and 35·4 meters above the mean sea surface (in the figures the heights are given in Norwegian feet); the lowest has a maximum breadth of 19 meters. Fig. 3 is a section at a point between Sandvik and Grepstad, where the three levels are 14·5, 34·1 and 38·5 meters. Fig. 4 represnts a section at Grepstad where there are only two levels, namely, 14·5 and 38·4 meters.

2. 3. 4.



It is concluded, in the first place, that the terraces and "strandlinien" do not, taken as a whole, follow definite levels. Some of them are local and are observed only for short distances, while others extend along for many miles. The latter are more typically developed, are more connected with definite levels, and may be traced as such for long distances in Northern Norway. The formation of these was probably in part determined by periodic changes in climate. The course of any particular line is nearly horizontal, whether it runs parallel with the coast, or extends from the coast into the interior, although the highest levels are found in the interior of the fiords. The conclusion of Bravais (1842) that these lines are not horizontal but rather rise in level toward the interior, upon which the idea of a gradual secular elevation of the land, joined with an unchanged level of the sea, has been in part based is not accepted as generally true. That the wearing action of the sea has been the only cause in producing the results observed is not regarded as probable, and this conclusion is supported by several arguments; what other forces were instrumental in producing the result is not distinctly stated. The formation of the "strandlinier" must have begun at the upper edge of the downward slope and the excavation gone on from above down while the land rose slowly in reference to the surface of the sea. The apparent elevation of the land is regarded as having gone on gradually and slowly and not suddenly and interruptedly. In general these changes in level which went on along the coast of northern Norway during the post-glacial time are believed to be most easily explained by the supposition of a changing level of the sea.

2. *On the substances obtained from some "Forts vitrifiés" in France.*—M. Daubrée has made a critical mineralogical and

chemical examination of materials obtained from several "Forts vitrifiés" in different parts of France. This name is given to the walls or to the simple debris of walls, whose materials have been fused together by the action of fire.

The substance obtained from the neighborhood of Argentan was of a dark greenish brown color, opaque, and resembled certain slags. A section examined under the microscope revealed the presence of large numbers of crystals of an octahedral mineral, probably spinel, and also crystals of melilite, both formed by the process of fusion. An analysis showed a considerable amount of alumina and of soda, leading to the inference that the fusion had been accomplished by adding marine salt to the aluminous silicate in the clays and schists. Some partially fused granitic rocks from the forts of Château-vieux and of Puy de Gaudy (Creuse), also from the neigborhood of Saint Brieuc (Côtes-du-Nord), were especially examined. The specimens consisted of small fragments of the granite, some angular, others more or less rounded, and all forming a solid mass, with a glassy surface. They were in some cases similar in appearance to volcanic scoria.

When sections of the granite were examined in the microscope it was found that the orthoclase still acted upon polarized light, and the albite also was nearly unaltered, but besides them there were vitreous masses produced by the fusion. Of the minerals formed by the process, spinel was very common in regular octahedrons, sometimes transparent, sometimes opaque. There are also large numbers of microlites in geodes in the fused mica, which are probably to be referred to a triclinic feldspar. The small quantity of fluorine originally contained in the mica is regarded as having played an important part in the changes accompanying the fusion. These granites had been fused immediately by fire without the aid of soda, as in the first case named, and it is reasonably certain that the process of fusing together the small fragments was intentional although the means by which it was accomplished so thoroughly is less easy to understand.

3. *Preglacial Outlet of the Basin of Lake Erie into that of Lake Ontario.*—Mr. J. W. SPENCER discusses this subject in a paper published in the Proceedings of the American Philosophical Society for 1881. He reaches the conclusion that a deep channel passed off from the southern part of Lake Huron along the course of the present valley of the Au Sable, pursued an east-southeast course and entered the basin of Lake Erie west of Vienna, bent around Long Island (east of Vienna), and then took a north-by-west course to Ancaster in the Province of Canada, whence it followed an easterly course along Dundas Valley into the west end of Lake Ontario; and that this channel was in preglacial time the outlet of Lake Erie into Lake Ontario. The supposed channel is now buried beneath drift. In the Dundas Valley (which is bounded by vertical escarpments) the drift has been penetrated to a depth of 227 feet below the surface of Lake Ontario. He also endeavors to show that the Great Lakes owe their existence to subaerial and

fluviatile agencies, but not to glacier excavation. The memoir is accompanied by two maps of the region.

4. *Laccoliths* (*or Laccolites*) *in Japan.*—Mr. G. H. Kinahan has described Laccolith-like intrusions of eruptive rocks in Counties Wexford and Wicklow, Ireland. They occur in highly disturbed Cambro-Silurian strata of different kinds, and the latter are baked or altered for some distance about them.—*Geol. Mag.*, March, 1881.

5. *Iron Ore of Iron Mine Hill, Cumberland, Rhode Island.*—Mr. M. E. Wadsworth describes this "titaniferous iron ore"—a titaniferous magnetite—as containing in its ground-mass large crystals of a triclinic feldspar along with *chrysolite* in grains, and mentions its resembling the Taberg iron ore rock of Sweden. Part of the chrysolite is changed to serpentine. An analysis of the ore by Prof. R. H. Thurston obtained 9·9 per cent of titanium. The rock nearest to the iron ore-bed is mica schist "some hundred feet away." Mr. Wadsworth supposes the iron ore to be of eruptive origin. (Mus. Comp. Zool., vol. vii (*Geol. Series, vol. I*).

It is of importance to note that a chrysolitic magnetite occurs at the O'Neil Mine, Monroe, Orange County, New York, the chrysolite of which was first determined and described by Prof. Brush, who gave it a distinctive name, hortonolite, on account of the amount of manganese present. As the iron ore deposits of Sussex County, New Jersey, and Orange County, New York, constitute *beds conformable to the adjoining schist*, and are, as Prof. G. H. Cook, of New Jersey, states, after extensive investigation, of metamorphic origin, it is probable that the Rhode Island magnetite is also metamorphic. J. D. D.

6. *Brazos Coal-field, Texas.*—A paper on this Texas coal-field, by C. A. Ashburner, is published in the Transactions of the American Institute of Mining Engineers, for 1881. The coal field proper in the southwestern part of the "Missourian or Fourth bituminous coal-basin of the United States" in which are two workable beds 2½ to 6 feet, are 85 feet thick, and are included between an upper sandstone and conglomerate, representative of the Millstone grit or Pottsville conglomerate (No. XII of the Pennsylvania series), and a lower gray limestone representative of the Mountain limestone, or Chester and St. Louis limestone, of the Mississippi Valley.

7. *Report of the Geological Survey of Pennsylvania, on the causes, kinds and amount of waste in mining Anthracite* (numbered A2), by Franklin Platt; with a chapter on the Methods of Mining, by J. P. Wetherell. 134 pp. 8vo, with thirty-five figures of mining operations, a plan of an anthracite breaker, also a specimen sheet of the work of the survey in the Anthracite Coal Field.—The specimen sheet of the anthracite coal-field, appended to this very important and well illustrated report, is by C. A. Ashburner. It contains a section on a large scale, exhibiting the stratification and flexures of the beds, and also a corresponding ground plan or map view, giving the topographical features of the

region, and the actual positions and structure of the several "veins" (beds), as explored.

Mr. Ashburner, in a paper read before the American Institute of Mining Engineers, states that his plan includes the exhibition on the sheets, besides surface features, underground contour curve lines of the chief coal-beds in the individual districts, the area worked out, and that under development of the contoured bed, all gangways, tunnels, adits, overlying and underlying the contoured bed, represented by a conventional color and line for each bed; so that the maps will give the areas worked out and undeveloped, the structure and positions of the beds, the amount of coal, and the probable structure of the undeveloped areas.

8. *Land-plants in the Middle Silurian of North Wales.*—Dr. HENRY HICKS describes (Proc. Geol. Soc., May 25, 1881), remains of Lycopodiaceous plants referred by him to Dawson's genus *Psilophyton*, spherical bodies resembling the *Pachytheca* of Sir J. D. Hooker, and numerous minute bodies supposed to be microspores of Lycopodiaceæ, besides seaweeds, from the Denbighshire grits, near Corwen, in North Wales. The associated graptolites were, according to Mr. Hopkinson, partly Middle and partly Upper Silurian forms, some being Llandovery species, here dying out, and others, Wenlock species, first appearing here.

9. *Vertebrata of the Permian Formation of Texas*, by E. D. COPE.—No. 32 of Prof. Cope's Paleontological Bulletin contains Plates I to IV of remains of *Eryops megacephalus*, V, of *Empedias molaris*, and VI of *Dimetrodon incisivus*, which are published also in the Proceedings of the American Philosophical Society, vol. xix, p. 56 (see this Journal, xxi, 407). In the American Naturalist for February last he has published a list of the Fishes, Batrachians and Reptiles of the Permian of the United States, numbering in all 51 species.

10. *Life-History of Spirifer lævis*, by Prof. Henry S. Williams, Ph.D.—Prof. Williams's paper on Spirifer lævis, an abstract of which is given in the twentieth volume (1880) of this Journal, has been published in full in the Annals of the New York Academy of Sciences, vol. ii, No. 6.

11. *Geological Society of London.*—At the annual meeting of the Geological Society in February, the Wollaston gold medal was presented to Prof. P. MARTIN DUNCAN; the Murchison medal to Prof. ARCHIBALD GEIKIE; the Lyell medal to Dr. J. W. DAWSON, of Montreal; the Bigsby medal, to Prof. MORRIS.

12. *On the Optical Characters and Crystalline System of some important Minerals.*—The results obtained by the more exact methods of investigation employed in mineralogy in the past few years have led to the change of a considerable number of minerals to systems of a lower grade of symmetry than those to which they had previously been assigned. The classical memoir of Mallard (Ann. des Mines, vol. x, 1876) has had a strong influence in this direction. In it he sustained this change for some of the best known species and those which had been accepted as types

of the systems to which they were referred; for example, garnet, vesuvianite, fluorite, apophyllite, zirkon, apatite, beryl, tourmaline, and so on. Mallard suggested, in explanation of cases like those named, the hypothesis that such crystals were to be considered as twins or compound crystals so made up as to have a pseudo-symmetry corresponding to a higher grade than that belonging to the individuals themselves.

The question as to the sharpness of the line dividing the crystalline systems from each other, and in many cases as to which system a given species really belongs, cannot be said to be decided at the present time. It is certainly possible to exaggerate the "optical anomalies" and to attribute to them a morphological significance when they are in fact due simply to accidental causes, such as the internal tension produced at the time the crystal was formed. For example, the species boracite, long held as a typical hemihedral form in the isometric system, although with an anomalous optical character variously explained, has by Mallard and others been referred to the orthorhombic system. Recently, however, Klein (Jahrb. Min., 1881, 239) has shown by the effect upon the optical character produced by heating sections of the crystals that the peculiarities are probably due to internal tension simply, and that there is nothing which really conflicts with its being referred to the isometric system. Similarly, analcite, the common form of which was long held to be a typical example of an isometric trapezohedron, was afterward referred to other systems by Schrauf, Mallard, Lasaulx and others, and finally referred back to the isometric system by several mineralogists who have reached the same result by somewhat different methods. Other similar examples might be given.

M. Bertrand, working from the standpoint of M. Mallard, has recently published some interesting contributions to this subject. He shows that the apparently isometric octahedrons of ralstonite exhibit two optic axes with an angle of about 90°. He has also examined a series of minerals ranging from the pure lead phosphate, pyromorphite, to the lead arsenate, mimetite; the conclusion is that while the first is truly hexagonal and has one negative optic axis, the other is really orthorhombic, and owes its apparent hexagonal form to twinning. A section of mimetite from Johanngeorgenstadt, cut normal to the vertical axis, was seen in polarized light to be made up of six triangles, each having as a base the side of the hexagon; the two optic axes make an angle of 64° in air. Between the two extremes there are various intermediate compounds containing both P_2O_5 and As_2O_5, and it is found that as the proportion of As_2O_5 diminishes, the angle of the optic axes also diminishes. Similar results have been obtained by M. Jannettaz. These facts recall the results obtained by Cooke with crystals of iodide of antimony, who proved the existence of a uniaxial (hexagonal) and a biaxial (orthorhombic) variety, of which the latter changes into the former on a slight elevation of temperature. M. Bertrand has also studied

several varieties of garnet and arrived at results essentially the same as those of M. Mallard. Sections of crystals of aplome, ouvarovite and topazolite show two optic axes with an angle of about 90°. The ouvarovite is regarded as made up of twelve pyramids having the faces of the crystal as their base and their vertices at the center. The dodecahedrons of aplome and topazolite are explained as formed of forty-eight simple crystals. Further than this, he found it possible to separate the dodecahedrons mechanically into these forty-eight individuals, each one of which is truly biaxial; the fracture-surfaces are smooth and make angles of 60° with the rhombic faces when the plane of separation obtained is parallel to the side of the rhomb, and of 90° when it is parallel to one of the diagonals. The former fractures are obtained more readily than the second, and it is concluded from this that the union of the four crystals which form together the same rhombic face is more intimate than that of the twelve complex rhombohedral pyramids among themselves. That this is the true explanation of these facts may perhaps be questioned.

13. *Brief notices of some recently described minerals.*—CHALCOMÉNITE. A new species described by M. DesCloizeaux, from the Cerro de Cacheuta, south-east of Mendoza, Argentine Republic. It occurs in transparent crystals, and in thin crystalline crusts of a blue color; it is associated with a compact mineral of a violet color, and having, according to M. Pisani, the composition $(Cu_2, Pb)Se$. The crystals belong to the monoclinic system and are generally combinations of the prism ($I \wedge I = 108° 20'$), and the orthopinacoid and basal plane ($i\text{-}i \wedge O = 90° 51'$). The plane of the optic axes is parallel, and the acute negative bisectrix perpendicular to the horizontal edge $O \wedge i\text{-}i$. The axial angle is small, and the ordinary dispersion ($\rho < v$) so great that with a green glass the lemniscates have the form of circular rings with a black cross, while with a blue glass they become elongated ellipses, normal to the plane of polarization of the microscope, and with the hyperbolas separated about 10°, at 45° with this plane. The composition of the new mineral has not been fully determined, owing to lack of material, but preliminary trials by M. Damour show it to be essentially a hydrated selenite of copper.—*C. R.*, April 4, 1881.

TRITOCHORITE. Described by Frenzel, locality unknown. Physical character as follows: Massive with columnar structure; cleavage longitudinal, tolerably distinct, yielding thin plates; H. =3·5; G. =6·25; color blackish brown, with lighter yellowish brown spots; streak pale lemon-yellow. An analysis yielded: V_2O_5 24·41, As_2O_5 3·76, PbO 53·90, CuO 7·04, ZnO 11·06 =100·17.—*Min. Petr. Mitth.*, iii, 506.

LAUTITE. Also described by Frenzel, from the mine Rudolfschacht at Lauta, near Marienberg, Saxony. Occurs massive with columnar, fibrous, or granular structure. H. = 3–3·5; G.= 4·96; luster metallic; color iron-black; streak black. An analysis gave: As 42·06, S 18·00, Cu 27·66, Ag 11·74 =99·40.

This corresponds with $Cu_4AgAs_5S_6$. Associated with native arsenic, pyrargyrite, chalcopyrite, tetrahedrite, galenite and barite. —*Ibid.*, p. 515.

SERPIERITE. M. Des Cloizeaux has given the name serpierite to a new mineral from the zinc mines of Laurium, Greece. It is found in minute dark greenish blue tabular crystals, belonging to the orthorhombic system. They have the base O broad, and show, also, the prism ($I \wedge I = 98° \ 42'$), the pyramid 1 ($O \mid 1 = 115° \ 32'$), and several brachydomes. The plane of the optic axes is parallel to the longer diagonal of the base; the axial angle for red glass is 44° 20′ in oil, or 67° 10′ for air. M. Bertrand has obtained similar results. According to preliminary trials by M. Damour, the mineral is a hydrous basic sulphate of copper and zinc.—*Bulletin Soc. Min. France*, iv, 89, 1881.

SCHNEEBERGITE. This mineral occurs in minute transparent octahedrons, of a honey-yellow color and vitreous to adamantine luster. H. =6·5; G. =4·1. It consists mostly of lime and antimony, with iron and traces of other elements; related to romeite. Found by Lhotsky on the Schneeberg in the Tyrol, associated with anhydrite (or gypsum), chalcopyrite and magnetite; partially described by Brezina.—*Verh. geol. Reichsanstalt*, 1880, No. 17.

"TYREEITE." One and a half hundred weight of the "carnelian marble" of Tyree, Scotland, dissolved in sixteen gallons of hydrochloric acid left as residue, thirty pounds of sahlite, a little scapolite and sphene, and some ounces of a *red mud.* By decantation 1·9 grams of powder of a deep brick red color was obtained. Of this mud, sulphuric acid dissolved ·78 grams, leaving 1·1 insoluble. The last was analyzed and decided to be an impure talc. The soluble portion yielded: Fe_2O_3 38·22, Al_2O_3 8·23, FeO 3·16, MnO 0·39, MgO 29·94, CaO 2·21, H_2O 12·47, P_2O_5 4·71, SiO_2 1·02 =100·35. To this last obviously heterogeneous substance the new name is provisionally given by Heddle; certainly no name was ever given with less reason.—*Mineralogical Magazine*, iv, 189, 1881.

FREDRICITE. A variety of tennantite, or arsenical tetrahedrite, from Fahlun, Sweden, described by H. Sjögren, peculiar in containing both lead and tin. H. = 3·5; G = 4·65; color iron-black; a brilliant metallic luster. An analysis gave: As 17·11, Sb *tr.*, S 27·18, Cu 42·23, Pb 3·34, Sn 1·41, Ag. 2·87, Fe 6·02 =100·16.—*Geol. För. Förh. Stockholm*, v, 82, 1881.

ARCTOLITE. A mineral described by Blomstrand, as collected in 1861, on "Nordskön" near Spitzbergen. It forms thin irregular plates in marble. H. = 5; G. = 3.03; colorless, or yellowish to greenish. An analysis gave: SiO_2 44·93, TiO_2 0·38, Al_2O_3 23·55, Fe_2O_3 1·24, CaO 13·28, MgO 10·30, Na_2O 1·73, K_2O 0·79, H_2O 3·54 = 99·74. It is probably to be regarded as an altered hornblende.—*Ibid.*, p. 210.

FRIGIDITE. A variety of tetrahedrite from the Valle del Frigido, Apuan Alps, described by D'Achiardi. It is usually

in compact granular masses, rarely crystallized. H. a little less than 4; G. =4·8; luster metallic; grayish-steel colored. An analysis by Dr. Funaro gave, after deducting 2·2 p. c. SiO_2 and calculating to 100: Sb 27·00, S 31·23, Cu 20·39, Fe 13·37, Ni 7·97, Ag 0·04, Zn *tr.* = 100.

DUMORTIÉRITE. Found sparingly in small crystalline grains in the gneiss of Beaunan, valley of the Azeron, south-east of Lyons. It has a bright blue color, and the specific gravity is 3·36. It has been examined chemically by M. Damour, and shown to be a silicate of aluminum, with perhaps the composition $[Al_2]_4Si_3O_{18}$; this point, however, is not entirely established. M. Bertrand finds that it has distinctive optical characters similar to those of andalusite.

14. *On the mineral Dawsonite from Tuscany.*—The rare species dawsonite, described by Dr. Harrington in 1874 as occurring sparingly near Montreal, has been found by M. Chaper at Piau Castagnaio in Tuscany and has been investigated by M. Friedel. It is found in thin plates radiated and formed of fine fibers in a quartzose rock impregnated with dolomite. An analysis gave: CO_2 29·59, Al_2O_3 35·89, Na_2O 19·13, H_2O 12·00, MgO 1·39, CaO 0·42. This corresponds closely with the results of Harrington but the material in hand seems to have been purer. This analysis agrees closely with the formula $Na_2[Al_2]C_2O_8+2H_2O$, which may be written $3(Na_2CO_3)+(Al_2C_3O_9)+2(H_6[Al_2]O_6)$.—*Bull. Soc. Min. France*, iv, 28.

15. *Vanadium minerals from Cordoba, Argentine Republic.*—The occurrence of vanadium minerals at several points in the State of Cordoba has been described by Brackebusch (Las Especies Minerales de la Republica Argentina, 1879). Crystals of descloizite and vanadinite from this locality have been figured by Websky (Ber. Ak. Berlin, July and October, 1880). He shows that the vanadinite has the same hemihedral characters as apatite, the crystals being highly modified and showing the planes, *O*, *I*, *i*-2, 1, 2-2, and 3-$\frac{3}{2}$. Rammelsberg (ZS. G. Ges., xxxii, 709) has analyzed the descloizite and obtained for dark colored crystals. G. = 6·080: V_2O_5 22·74, PbO 56·48, ZnO 16·60, MnO 1·16, H_2O 2·34, Cl 0·24. This corresponds to the formula $R_4V_2O_9$ or $R_3V_2O_8$ + RH_2O_2 with R=Pb : Zn=1 : 1, and makes the species analogous in composition to olivenite and libethenite.

Occurring with the descloizite and vanadinite is a mineral in small black striated prismatic crystals, for which Dr. Döring proposes the name *Brackebuschite.* An analysis by him yielded:—V_2O_5 25·32, P_2O_5 0·18, PbO 61·00, MnO 4·77, FeO 4·65, ZnO 1·29, CuO 0·42, H_2O 2·03=99·66. For this Rammelsberg calculates the formula $R_3V_2O_8$+aq., which (Mn : Fe=1 : 1) requires:—V_2O_5 25·45, PbO 62·09, MnO 4·95, FeO 5·01, H_2O 2·50=100.

16. *Zinn: Eine geologisch-montanistisch-historische Monografie;* von E. REYER. 248 pp. 8vo. Berlin, 1881 (G. Reimer).—Dr. Reyer has already published a series of papers devoted to the subject of tin, treated both from the geological and the technical

standpoint. In this volume he has brought together a very large amount of useful matter. The introduction contains a very complete list of memoirs previously published. The subject is divided geographically, the tin-mining districts of Bohemia and Saxony coming first, then those of Cornwall, Burma, Siam, Australia and Tasmania. The geology is treated briefly and concisely, in part in connection with each locality, and more fully in a résumé at the close. The larger part of the volume, however, is devoted to a history of the tin-mining, and here much valuable information has been brought together.

III. Botany and Zoology.

1. *Marine Algæ of New England and adjacent coast;* by W. G. Farlow, M.D. (Reprinted from Report of U. S. Fish Commission for 1879.) Washington, 1881. 8vo, pp. 210, tab. xv.—Hitherto the only good work attempting to describe all the seaweeds of our coast has been the *Nereis Boreali-Americana* of Dr. Harvey, published by the Smithsonian Institution in three quarto volumes from 1852 to 1858. This work took in the seaweeds of the entire coast of the United States, Pacific as well as Atlantic. Since it appeared the industry of collectors has detected many additional species along the several portions of our coasts, and the acumen of Phycologists at home and abroad has given us much information respecting the true structure, physiology and affinities of many of the forms already known. In the present work Professor Farlow has limited himself to the seaweeds known on or near to the coast of New England; but the systematic classification which he has adopted, and the many interesting and new points of structure and function which he has either discovered for himself, or has accepted from the writings of Thuret, Bornet, Agardh, Le Jolis, Rostafinki, etc., render the work a valuable text-book for the study of marine Algæ wherever the English language is read.

The old classification of Algæ was into the three sub-classes of *Melanospermeæ*, *Rhodospermeæ* or *Florideæ* and *Chlorospermeæ*. Within thirty years the disposition of the *Florideæ* has met with no fundamental change, but the *Melanospermeæ* and the *Chlorospermeæ* are no longer recognized in their integrity. The sexual reproduction of the *Fucaceæ* is now as well understood as that of *Rosaceæ*, and is clearly of an oosporic character; that is, the unfertilized nucleus is fertilized by antherozoides after exclusion from the conceptacle. But no sexual reproduction of *Laminarieæ*, *Punctarieæ*, *Dictyosiphonieæ*, *Desmarestieæ*, and their allies has been detected, and it is very doubtful whether any exists. These tribes are therefore grouped in the suborder *Phæosporeæ*, and united with *Chlorosporeæ*, *Bryopsideæ* and *Botrydieæ*, all grass-green Algæ, in the order *Zoosporeæ*.

The obscure *Chroococcaceæ* and *Nostoclineæ* form also a separate order, the *Cryptophyceæ*. Thus Professor Farlow recognizes four orders, *Cryptophyceæ*, *Zoosporeæ*, *Oosporeæ and Flori-*

deæ. An ample explanation of this system is given in the twenty-four pages of introduction, where one may look in vain for a recognition of the conjoining of Algæ and Fungi in chlorophyllose and achlorophyllose branches of common classes, as was proposed by Cohn and Sachs, and set forth in Professor Bessey's "Botany for High Schools and Colleges." It is certainly pleasant to the student of Algæ to be relieved from having to consider the objects of his study not a distinct class or group of classes, but only chlorophyll-bearing equivalent of Fungi, and so but halves of an uncomprehended and ill-assorted whole! It is still more reassuring when this relief comes from the laboratory of one so learned in the physiology of both Algæ and Fungi as Professor Farlow.

In the quarter of a century which has elapsed since the appearance of Harvey's *Nereis*, but one conspicuous red Alga has been discovered on our coast and that one is *Nemastoma Bairdii*, of which Professor Farlow found but a solitary specimen at Gay Head in 1871. The principal changes in nomenclature among the *Florideæ* are the substitution of *Rhabdonia tenera* for *Solieria chordalis*, of *Rhodophyllis Veprecula* for *Calliblepharis ciliata*, and of *Griffithsia Bornetiana* for *G. corallina. Rhodomela gracilis* and *R. Rochei* are reduced to forms of *R. subfusca* and *Polysiphonia formosa* to *P. urceolata.* Harvey knew certainly of no Coralline on the New England coast. But *Corallina officinalis* is very common, and Professor Farlow has recognized, in addition, five species of *Melobesia* and two of *Lithothamnion*. There are two or three added species of *Fucus*, and *F. nodosus* is excluded from the genus, to become *Ascophyllum nodosum. Sargassum Montagnei* is very properly retired. Among the great *Laminariæ* are some changes: *L. dermatodea* and *L. borea* together constitute *Saccorhiza dermatodea*, the genus differing from *Laminaria* principally in the form of the hold-fast and in the presence of cryptostomata. *L. platymeris* is added, *L. saccharina* and *L. digitata* retained, though with some apparent hesitation, *L. longicruris* fully recognized, and *L. Fascia* referred to the genus *Phyllitis* in *Scytoriphonieæ.*

There are several changes among the filamentous *Chlorosporeæ*, and still more that is new (to Americans at least) among the membranous forms, the genus *Monostroma*, with four species, being introduced, and the species of Ulva arranged after Le Jolis in the "Liste des Algues Marines de Cherbourg." The *Cryptophyceæ* were but indistinctly known to Dr. Harvey, but are now satisfactorily arranged in sixteen genera, among which only *Oscillaria*, *Microcoleus*, *Lyngbya*, *Calothrix* and *Rivularia* are given in the Nereis.

The Diatoms and Desmids are not treated of in this work.

The fifteen plates at the end of the volume are mainly illustrative of the different kinds of fructification seen in Algæ, and add much to the ease with which one may comprehend the principles of classification here set forth.

It is to be hoped that the able botanist who has given us this most important contribution to the history of North American

Algæ will before long publish a similar report on the seaweeds of the Pacific Coast, and then a comprehensive work on all North American Marine Algæ. D. C. EATON.

2. *Das System der Medusen von E. Hæckel; Zweite hälfte.*—The conclusion of the first part of "Hæckel's System der Medusen," devoted to the Acraspedæ, Steganophthalmæ, or the Discophoræ in their widest sense, has been issued.

Though some of the orders adopted by Hæckel differ but slightly from those previously recognized, they are invariably baptized anew, and we find in this, as in all the systematic work of Hæckel, a deliberate disregard of the nomenclature adopted by his predecessors. Hæckel stretches the laws of nomenclature to their extreme limits, and nothing can render them more ridiculous than such a systematic nomenclature as that of the System d. Medusen. Every genus, every family, every order, in fact, every division or subdivision adopted, invariably receives a new name if its limits are either greater or smaller than those of the corresponding division previously known to science. The same principle would warrant us in rebaptizing any well-known animal, provided some important point of its structure, unnoticed heretofore, were described in detail and made to form the basis of the new-fangled name by which it is hereafter to be known. Nomenclature is properly an aid in ascertaining the views of our predecessors, and in limiting and in defining the existing state of the knowledge of a group; its main object is not the introduction of new terms, and an endless confusion, merely in order to glorify the peculiar systematic views of the latest philosophical writer on the subject. This defect to which we had already called attention in the first part of the System is far more prominent in the second part, where the material is less complete, and is derived, for the greater part, from alcoholic specimens of Medusæ which Hæckel has had no opportunity to study from life. We may close this part of the subject by asking Medusologists what is gained by the fabrication of such names as Stauromedusæ and Cubomedusæ?

Hæckel adopts, with Huxley and nearly all the later writers on Medusæ, the group of Lucernaridæ, and one of his most interesting new types is the genus Tessera (from specimens collected by the Challenger). This genus shows the close systematic affinity existing between the Lucernaridæ proper and the true Discophoræ. It is, in fact, nothing but a free Lucernaria. Closely allied to them are the Peromedusæ. To this group belong a number of large Medusæ, which probably live on the bottom in deep water. Several species were collected by the "Challenger," and the "Blake"* dredged off the N. E. extremity of George's Bank a number of specimens of Dodecabostricha (Brandt), Periphylla (Steenst.).

Hæckel establishes (from alcoholic specimens) several genera and families of this interesting group of Medusæ, and gives an

* See Bull. M. C. Z., vol. viii, No. 9, 1881.

excellent anatomy of their more prominent details, hitherto only known from the drawings of Mertens and the descriptions of Steenstrup. It seems to us as if Hæckel had needlessly multiplied not only the families, but even the genera of this group. (Compare Pericolpa and Periphylla.)

Among the Charybdeidæ we must call special attention to the interesting genera Procharybdis and Chirodropus. These are specially important as bringing the Charybdeidæ into closer systematic relationship to the other Discophoræ.

In the next order, the Disco-Medusæ, he adopts the primary subdivisions of the group Semeastomæ and Rhizostomæ proposed by Agassiz. Although he prefaces his review of that classification by stating that it is entirely unnatural, he at once, after removing some of the forms included in these divisions into other families, proceeds to adopt it. Hæckel makes a most characteristic attempt to show that Agassiz willfully neglected to quote Huxley's paper on the anatomy and affinity of the family of the Medusæ. (See p. 27 Contrib. Nat. Hist. U. S., vol. iii, where Huxley's paper is quoted.) Naturalists who willfully ignore or misrepresent the work of their colleagues are fortunately more rare than those who are known to manufacture drawings to suit their pet theories. Hæckel, of course, differs from Agassiz radically in his estimate of the value of the homology between Acalephs and Echinoderms. His view may be "grundfalsch" according to Hæckel, but it certainly is not yet so considered by those embryologists who have the best right to an opinion on the subject.

The first subdivision adopted by Hæckel (in addition to those mentioned above), the Cannostomæ, can hardly be considered creditable to a zoologist having so extensive a knowledge of Acalephs as Hæckel. This subdivision is based entirely upon a few alcoholic specimens of Discophoræ, any one of which may turn out to be the young stage of some unknown Discophoræ. Of the Cannostomæ, Hæckel has examined only two species from living specimens; the other sixteen are based upon alcoholic material, which, no matter how well preserved, will not give even a Hæckel an idea of their ontogeny. Among the Semeastomæ we find the new family of Flosculidæ, including Floscula and Floresca—genera which are probably closely allied to embryonic Pelagiæ and the new family of Ulmaridæ: the genera Ulmaris, Umbrosa and Undosa, allied to embryonic Aureliadæ.

Among the Rhizostomidæ Hæckel gives, with other new genera, good figures of Archirhiza and of the family of Versuridæ (Versura, Cannorhiza). Among the Crambessidæ, a family which Hæckel established in 1869 upon a new species of Rhizostoma, are illustrations of Leptobrachia and Thysanostoma. This same species of Crambessa (*C. Taji*) subsequently formed the subject of an excellent monograph by Grenacher and Noll, which added greatly to our knowledge of the Rhizostomæ.

The majority of the plates of the second part of Hæckel's

Acalephs are drawn from the alcoholic material which was placed at his disposal by nearly all the European museums. These illustrations suffer as compared with those of the Hydroids, where Hæckel had a large amount of new, fresh material at his disposal.

The value of this monograph is, however, very great, as it has cleared the ground of a great deal of rubbish and will enable the future investigator to work upon a comparatively firm basis.

Discophorous Medusæ are by no means as common as Hydroids; their habits are as yet but little known; though they are often found in swarms upon the ocean, it is usually under circumstances which render their capture or detailed examinations at the time impossible. I well remember laying off the Bar of San Francisco for a number of days and seeing the greater number of the species of Discophoræ, so well figured by Mertens, float by out of reach, only near enough to be roughly identified, while it was impossible, on account of the rolling of the schooner, to examine properly the few I was fortunate enough to capture. Fortunately, as Hæckel's monograph has well shown, a great part of their structure can be made out from carefully preserved alcoholic specimens, and until some naturalist, under more favorable circumstances, gives us anatomical details drawn from life, to these we must look for the principal additions to our knowledge of the Discophoræ.

A list of the fossil Medusæ thus far described is added to the volume, and a few appendices, making corrections and additions of imperfectly known Medusæ. It closes with a final appendix containing a puerile attack on Metschnikoff, evidently suggested, as Hæckel naively says, by the fact that "Metschnikoff bei jeder Gelegenheit meine zoologische Arbeiten auf das heftigste schmäht und angreift," and that Metschnikoff insists, with other Russian naturalists, in writing in his own language. It certainly is a pity that Russian naturalists will not follow the example of the Scandinavians and give us French or English résumés of their memoirs. But no nation, least of all the German, has a right to ask the most active embryologists of the present day to write to suit their convenience. The day may yet come, in spite of Hæckel, who evidently does not appreciate Chinese and Japanese civilization, when their investigators also will have as good a right to be heard as the Russians by all except the close corporation of naturalists of whose claims Hæckel is the exponent.

A. AG.

3. *New and little known Reptiles and Fishes in the Collections of the Museum of Comparative Zoology;* by S. Garman. Bull. Mus. Comp. Zool., vol. viii, No. 3. pp. 85–94. Cambridge, 1881.

4. *On the Results of Dredging under the supervision of Alexander Agassiz along the Atlantic Coast of the United States during the Summer of* 1880 *by the Steamer Blake.* Report on the Cephalopods, and on some additional species dredged by the U. S. Fish Commission Steamer Fish Hawk in 1880, by A. E. Verrill. Ibid., vol. viii, pp. 95–230. Also, Report on the Selachians, by S. Garman. Ibid., pp. 231–284.

5. *Arrangement of the Perissodactyles, with a note on the Structure of the foot of Toxodon*, by E. D. COPE.—Proceedings of the American Philosophical Society, April 15, 1881.

IV. ASTRONOMY.

1. *Photographic Spectrum of Comet* 1881, *b;* by WM. HUGGINS. —On Friday night (June 24th), I obtained with one hour's exposure a photograph on a gelatine plate of the more refrangible part of the spectrum of the comet which is now visible. This photograph shows a pair of bright lines a little way beyond H in the ultra violet region, which appear to belong to the spectrum of carbon (in some form) which I observed in the visible region of the spectra of telescopic comets in 1866 and 1868. There is also in the photograph a continuous spectrum in which the Fraunhofer lines can be seen. These show that this part of the comet's light was reflected solar light.

This photographic evidence supports the results I obtained in 1868, showing that comets shine partly by reflected solar light, and partly by their own light, the spectrum of which indicates the presence in the comet of carbon, possibly in combination with hydrogen.—*Communication from the Author; also Nature*, June 30.

2. *Notice of the Comet;* by CHARLES E. BURTON.—At about 11h. 0m. G.M.T. on June 29, a transit of the "following" nuclear jet of the great comet over a star of 8m. was observed by Mr. N. E. Green, of 39 Circus Road, St. John's Wood, and by me, with a 12½-inch reflector belonging to Mr. Green. Definition was very good and tranquil. As the star became involved in the jet it gradually increased in size, and, when seen through the brightest part of the jet traversed, resembled an ill-defined planetary disk about 3″ in diameter. At this moment the comet seemed to have two nuclei similar in aspect and brightness.

The effect of the cometary matter on the star's image resembled that of ground glass, not that of fog; the image of the star, being dilated into a patch of nearly uniform brightness, instead of presenting a sharp central point with a surrounding halo. Cirro-stratus, passing into rain-cloud, produces on the appearance of the sun an effect the counterpart of that produced by the cometary emitted matter on the star. There was not sufficient light for the use of the spectroscope, the star, afterwards identified as B.D. +65°, 519, being fainter than 8m.

The transit of the jet occupied about 3m. and the star slowly resumed its ordinary appearance and dimensions, the image *contracting* as the center of the jet left the star behind. A transit of this kind has not frequently been observed, at least under such favorable conditions as to brightness and definition of the objects, and it is to be hoped that others may have been as fortunate as Mr. Green and the undersigned.

If the point, which obeys the Newtonian law, be a solid body, the observation just recorded seems to show that its true outline

would probably be rendered unrecognizable, and its aspect totally altered by the (refractive ?) power of the coma and jets.—*Nature*, July 7.

3. *Observation on the Comet;* by W. H. M. CHRISTIE, made at the Royal Observatory, Greenwich.—Further measures have been obtained at Greenwich of the position of the least refrangible edge for three of the four comet-bands with the following results:—

	Yellow band.	Green band.	Blue band.
Comet	5630·4 ± 1·6	5162·7 ± 0·4	4733·9 ± 1·1
Bunsen Flame	5633·0	5164·0	4736·0
No. of Obs.	7	26	6

The identity of the comet-bands with those in the first spectrum of carbon appears to be clearly established, but in each case the comet-band is slightly shifted toward the blue. The displacement of the green band, if real, would indicate an approach of 47±14 miles per second, whereas the comet was actually receding from the earth at the rate of about twenty miles per second. Such a displacement might, of course, be explained by an emission of cometary matter on the side toward the earth, but it would seem more probable that it is due to the circumstance that the edge of the comet-band is not quite sharp, and that a small portion on the red side is cut off. This would apply with still more force to the yellow and blue bands, which indicate somewhat larger displacements toward the blue. The displacements however, though all in the same direction, are not largely in excess of the probable errors. The comet-bands were compared with those given by vacuum-tubes containing cyanogen and marsh-gas, as well as with those of the Bunsen-burner flame, and three forms of spectroscope were used, viz: (1) the half-prism spectroscope with a dispersion of 18½° from A to H, and a magnifying power of 14; (2) the half-prism spectroscope reversed (as for prominence observations), giving a dispersion of 5° from A to H and great purity of spectrum, with a magnifying power of 28; and (3) the star spectroscope with a single prism of flint. No measures were obtained of the band in the violet, which was only seen on two occasions. It appeared to be sensibly coincident with the band in the first spectrum of carbon at 4311.—*Nature*, July 14.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *International Polar Stations occupied by the Signal Service.*—The head of the U. S. Signal Service, General Hazen, has issued circulars from which are taken the following facts.

The permanent station will be established at the most suitable point near Point Barrow, Alaska (71° 27′ N., 156° 15′ W., as determined by Beechey). Meteorological, magnetic, tidal, pendulum and other observations of a physical kind are to be made and also collections gathered, as complete as possible, in mineralogy, botany, zoology and ethnology. This station will be visited in 1882, 1883, and 1884 by a steamer or sailing vessel, to furnish supplies and such additions to the party as may be necessary.

The officers assigned to duty as the expeditionary force are Lieut. P. HENRY RAY, of the 8th Infantry, Acting Signal Officer, and Commander of the Expedition; G. S. OLDMIXON, U. S. Army, Acting Assistant Surgeon; Sergeants J. MURDOCH, U. S. A., and MIDDLETON SMITH, U. S. A., Naturalists and Observers; Capt. E. P. HERENDEEN, Interpreter, Storekeeper, etc.; Mr. A. C. DARK (of the Coast Survey), Astronomer and Magnetic Observer.

The meteorological and tidal observations will be made at exact hours of Washington civil time—the longitude of the Washington Observatory being $5^h\ 8^m\ 12^s{\cdot}09$ west of Greenwich; and the regular magnetic observations at even hours and minutes of Göttingen mean time—Göttingen being in $0^h\ 39^m\ 46^s{\cdot}24$ east of Greenwich, or $5^h\ 47^m\ 58^s{\cdot}33$ east of Washington. The equipment in instruments for the various kinds of physical observations is to be very complete.

2. *Annual Report of the Board of Regents of the Smithsonian Institution, for the year* 1879. 632 pp. 8vo. Washington, 1880.—The Smithsonian Institution is ably fulfilling its purposes under Professor Baird, in the various ways established during the administration of Professor Henry. This Report gives an account of what it is doing in the way of aiding and extending research, and explorations, making collections and sustaining the National Museum, carrying forward the objects of the Fish Commission, making exchanges in specimens, transporting exchanges in publications between this and foreign countries and by various other methods. Pages 143 to 212 are occupied with a memoir on James Smithson and his bequest. Next follows the General Appendix containing many Archæological and Ethnographic papers, occupying 270 pages, and also, Reports of American Observatories by Prof. E. S. Holden, and translations of a memoir by Dr. F. J. Pisko on the present fundamental principles of Physics, and another by E. H. Von Baumhauer, Permanent Secretary of the Netherland Society of Sciences, Harlem, on a Universal Meteorograph designed for detached Observatories.

3. *Endowment of the American Chemical Society.*—An effort is now on foot, and vigorously pushed, to secure an endowment for the maintenance of the American Chemical Society. The sum proposed to be raised is fifteen thousand dollars, and a list published in the Philadelphia Inquirer, July 6th, shows that about one-half this sum has already been subscribed. Professor ALBERT R. LEEDS of Hoboken is Chairman of the Endowment Fund Committee, and receives notices of subscriptions. This laudable effort will, when complete, place the publication of the Journal of the Society upon a safe basis. The chemical manufacturers of the United States are a wealthy body, and we notice with pleasure that some of them have responded liberally to this call, as indeed they can well afford to do. There are but few men of wealth among the chemical investigators, but the names of several of the leading chemical teachers are recorded as subscribers to this fund; and aid from others is invited.

4. *Dr. J. Lawrence Smith's Collection of Minerals and Meteorites.*—We learn from the correspondence published in the Louisville Courier-Journal of July 12th, that Dr. Smith has presented his minerals and meteorites to the "Polytechnic Society" of Louisville, Kentucky. This society already possessed the well known mineralogical collection formed by the late Dr. Troost of Nashville University. The collection of meteorites formed by Dr. Troost, and for the most part described by him, was separately secured by Dr. Smith, and he had added largely to it by his own researches and exchanges. The collection thus increased now returns, as we understand, to the Troost cabinet.

Dr. Smith's gift to the "Polytechnic" includes also a collection of physical apparatus, which will now be in the custody of Dr. Tobin, who is entirely devoted to its preservation and scientific usefulness.

OBITUARY.

ACHILLE DELESSE.—The death of Delesse is mentioned on page 416 of the May number of this Journal. Delesse's researches in science were chiefly in the departments of mineralogy and geology. His labored memoirs on metamorphism and pseudomorphism, and his investigations with regard to the chemical constitution and other characters of various kinds of rocks, contributed largely to the progress of lithology and geology. He experimented also with important results on the expansion of rocks by heat and fusion, the magnetic properties of rocks, their absorption of moisture and its effects on their resistance to crushing, and on other points. In connection with the results of the Exposition at Paris of 1855, he produced a very valuable work entitled "*Matériaux de Construction;*" and he later published memoirs illustrated by large charts, on the constitution of the bottom of the seas, and on the soils, underground water-plain, and subsoils, about Paris. His "Revue des Progrés de la Géologie," prepared for the "Annales des Mines," but lately with the aid of M. de Lapparent, reached its sixteenth volume during the past year. Delesse, in 1845, was placed in the chair of Mineralogy and Geology at Besançon, and in 1850, in that of Geology, at the Sorbonne, at which time he was made "Ingénieur des Mines," and had charge of the quarries of Paris. Eighteen years later he was made Professor of Agriculture at the École des Mines. In 1878, he was promoted to Inspector-General of Mines, and placed in charge of the southeast division of France. Delesse was elected a member of the Academy of Sciences in 1879. M. Daubrée closes as follows his remarks at the funeral, on the 29th of March: "The breadth of mind and uprightness of Delesse, his astonishing powers of work, his learning, his kindness of heart, associated with true modesty and great loyalty of character, have made him esteemed and beloved during all periods of his useful career."

ETIENNE HENRY SAINTE CLAIRE DEVILLE, the eminent French Chemist, died at Boulogne-sur-Seine, on the 1st of July, having passed his 63d birthday in March last.

THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. XXIX.—BENJAMIN PEIRCE.*

BENJAMIN PEIRCE was born in Salem, Mass., on the 4th day of April, 1809, and he died at Cambridge, on the 6th day of October, 1880.

In his early years he had the good fortune to come under the influence of Doctor Nathaniel Bowditch. It is said that their first acquaintance was made while Dr. Bowditch's son Ingersoll and young Peirce were schoolmates. Ingersoll showed his comrade a solution which his father had prepared of a problem that the boys had been at work upon. Some error, real or conceived, was pointed out in the work, which was reported by Ingersoll to his father. "Bring me that boy who corrects my mathematics!" was the invitation to an acquaintance, the importance of which in Professor Peirce's own estimation is told in the dedication, more than thirty years later, of his "Analytic Mechanics" "to the cherished and revered memory of my Master in Science, Nathaniel Bowditch, the father of American Geometry."

Peirce entered Harvard College in 1825. As Doctor Bowditch was now in Boston, having removed from Salem in 1823, and was preparing the first volume of his translation of Laplace's "Mécanique Céleste" for the press, it followed almost as a matter of course that the college student was more influenced in his studies by him than by the college course. Doctor Bowditch's first volume was completed and the second entered for

* The Journal is indebted for this memoir to advance sheets from the Proceedings of the American Academy of Arts and Sciences, Boston.

copyright in 1829, the year of Peirce's graduation, and the proof-sheets were regularly read by him.

After graduation, two years were spent by Professor Peirce in teaching at Northampton. In 1831 he was appointed Tutor in Harvard College, and in 1833 was made Professor of Mathematics and Natural Philosophy.

The earlier years of his professorship were fruitful as to publication, principally in a series of text-books for use in college. The first that appeared were treatises on "Plane and Spherical Trigonometry" in 1835 and 1836, which were published in a more complete form, with a "Spherical Astronomy," in 1840. Next came a "Treatise on Sound," in 1836, which was based upon Herschel's work in the "Encyclopædia Metropolitana," but with very important changes. The bibliography of the subject in the Introduction is of permanent value. This was followed, in 1837, by his "Plane and Solid Geometry," and by a "Treatise on Algebra."

A work on "Curves, Functions and Forces" was begun in 1841 by the publication of a volume on "Analytical Geometry and Differential Calculus." A second volume, on the "Calculus of Imaginaries, Residual Calculus, and Integral Calculus," appeared in 1846. As the word "forces" in the title shows, he intended to complete this work by a third volume on the "Calculus of Variations, and on Analytical Mechanics, with its Applications," but in this form it was never done.

Instead of this, however, and so to be mentioned in this place, though not properly a text-book, there appeared in 1855 the "Analytic Mechanics" in a quarto form, a work that more adequately expresses Professor Peirce's peculiar power than any other of his productions, with perhaps one exception.

In all of these books he departed not a little from the beaten path. In geometry the idea of direction was made the basis of the theory of parallels. Infinites and infinitesimals are introduced, along with the axiom, "Infinitely small quantities may be neglected." The demonstrations are given only in outline, being in respect of fulness the entire opposite o Euclid. A like brevity is characteristic of the other books, and in fact of everything mathematical that Professor Peirce ever wrote. He used a notation to which he gave much thought, by which his formulas were more concise than they could easily be made with the usual symbols. The Integral Calculus was at the period of its appearance much in advance of similar works, especially in the treatment of differential equations. It is an excellent example of Professor Peirce's concise and logical style.

The "Analytic Mechanics" was rather a treatise than a text-book. In it Professor Peirce set forth the general principles and methods of the science as a branch of mathematical theory,

and embodied in a systematic treatise the latest and best methods and forms of conceptions of the great geometers. He aimed to reduce them to their utmost simplicity by freeing them from every superfluous element. He made free use of the idea of the *potential*, developing nearly the whole subject from it. Determinants are used regularly as a standing instrument of analysis, and especially in the integration of the differential equations of motion. Both of these features, as well as Jacobi's method of integration by his principle of the last multiplier, were at the time new in English treatises.

The whole volume is marked by a directness of thought and a brevity of expression which make it difficult reading for those who have been accustomed only to the usual forms of notation and reasoning, and who do not read the book in course from the beginning. Several of the chapters are made peculiarly interesting by the development of a large number of special problems as particular cases of general theorems. In his later years the author often said he wanted to rewrite the "Analytic Mechanics" and introduce quarternions into it.

In 1842 Professor Peirce published, in connection with Professor Lovering, four numbers of the "Cambridge Miscellany," a quarterly journal devoted to mathematics, physics and astronomy.

In the same year he assumed the care of the mathematical part of the "American Almanac," ten volumes of which were prepared by him. In one of these (1847) he published a list of the known orbits of comets, arranged in convenient form, to which he added to the usual cometic catalogue several approximate orbits computed by him for historic comets that had been imperfectly observed.

In 1849 Congress established a Bureau for the publication of the "American Ephemeris and Nautical Almanac," under the superintendence of Lieutenant (afterwards Admiral) Davis. Professor Peirce was at once appointed Consulting Astronomer. In this capacity he prepared and published, in 1853, his "Tables of the Moon," which have been used in making the "Ephemeris" up to the volume for the year 1883. In coöperation with Lieutenant Davis, he designed the form and general plan of the Ephemeris, and he decided upon all the coefficients to be used. He commenced a revision of the theory of the planets, especially the four outer ones; but this seems not to have been carried to serviceable results, if we except certain separate communications to this Academy. He retained the position of Consulting Astronomer until 1867. The high place which the "American Ephemeris" has ever held among like publications owes much to the character given to it by Professor Peirce in these its earliest years.

When, in 1846, Galle discovered the planet Neptune in the place pointed out to him by Leverrier, Professor Peirce took the liveliest interest in the admirable researches of Leverrier and Adams. He entered with zest into all the questions which were thus raised. What is the orbit of the new planet? What its mass? How much do they differ from the assigned orbits and masses? Does the new planet explain all the irregularities of Uranus? Did the data lead necessarily to the assigned place, and to it alone?

The results of his investigations were at various times given to this Academy, but more especially on the 4th of April, 1848. He then gave the perturbations of longitude and radius vector of Uranus by Neptune, and announced that Neptune and either of the two hypothetical planets of Leverrier and Adams would equally explain the observations of Uranus, within reasonable limits of error.

Leverrier had proposed to himself to solve the following problem:—From the observed irregularities of the planet Uranus to compute the elements of the orbit of an assumed exterior planet that has caused these irregularities. He ought perhaps to have limited himself to the other problem, to which he gave so correct an answer, Where among the stars astronomers must look in order to see the disturbing body. The elements of the orbit could be had from observations when once the planet was seen. He found for the unknown planet an orbit and a mass by processes that will always command the admiration of men; and the place in that orbit, as is well known, was less than one degree, as seen from the earth, from the actual place where Galle found Neptune.

Yet Professor Peirce declared that Leverrier's geometric planet and Neptune were not the same bodies. He praised without question the work of Leverrier and of Adams, asserting for them their right to all the praise and *éclat* which the world had given them. But Leverrier had distinctly stated that the planet which disturbed Uranus could not be at a less mean distance from the sun than 35; that is, that no planet that was within this distance could cause the observed irregularities of the motion of Uranus. Neptune, however, is at a distance of only 30, and does account for the perturbations of Uranus.

In this and in other communications Professor Peirce claimed that the perturbations changed their character at the points where the mean motions had the ratios 2 : 5 and 1 : 2, and that the reasonings of Leverrier were thereby vitiated. Not a little controversy has come from these papers of Professor Peirce; and we cannot say that the last word in regard to the question has even yet been spoken. As is not unusual in like discussions, there is probably a portion of truth and a portion of error

with either party. Leverrier and Adams each, as Professor Peirce has himself shown, by his own laborious researches, did point out correctly a place where a planet should be looked for, and assigned paths which that planet could have been traveling for more than one hundred and twenty years previously, and have caused the observed irregularities. Yet the elements of that planet's orbit and its mass and those of Neptune differ widely enough to justify the assertion that for the latter *they* were not correctly given.

On the other hand, astronomers will not probably agree with Professor Peirce in regarding the change of character of the perturbations when the mean motions of the new planet and of Uranus pass through the exact ratios 2 : 5 and 1 : 2 as of vital importance. In the usual form of development these fractions do indeed make certain terms infinite. That belongs, however, to the form of the development, not to the perturbations. In solving the question, "Where is the disturbing body?" the solution need not have involved these forms; and it has not been shown that they entered into the work of either Leverrier or Adams in such a way as to vitiate it.

That the problem was really indeterminate has been steadily held by Professor Peirce. In January, 1878, he read to this Academy a paper, which has not been published, and the conclusions of which, therefore, will not compel the assent of astronomers until some one else shall have gone over the same questions. He showed a chart of the plane of the ecliptic with the orbits of Uranus and Neptune, and having those parts of the plane shaded within any part of which a planet of arbitrary mass might have been situated in September, 1846, and yet have caused, in the preceding years, the observed irregularities in the motions of Uranus, within reasonable limits of error. With a circular orbit, a large fraction (more than one half) of the ecliptic, as seen from the earth, contained some of the shaded portions. If an eccentricity not greater than one-tenth be allowed, the region was greatly enlarged. While, therefore, the solutions of Leverrier and Adams gave a place and a path that explained the disturbances, the problem in its nature was not, he claimed, one having a single answer, or even a finite number of answers.

In 1852, Professor Bache, then Superintendent of the United States Coast Survey, induced Professor Peirce to take up the subject of the longitude determinations in the Survey. As a result, there appeared in the successive volumes of the "Coast Survey Reports," communications from him upon the several questions that arise in the treatment of that subject. The most noteworthy referred to the determination of our longitude from Greenwich, since local differences were determined by the tele-

graphic method. The whole subject of errors of observations, the law of facility of error which is assumed in the method of least squares, its limits and defects, and the habits of observers, were carefully examined. The method of occultations was decided to admit of greater accuracy than any other that was then available, and the occultations of the Pleiades to furnish the most convenient means of its application. Formulæ and tables were prepared, old observations collected, and new ones made to apply this method. The question of our longitude is now, thanks to the ocean telegraph, one of history; but the questions of errors in observing, which Professor Peirce so thoroughly treated, will always be of practical import.

It seems as though there was a connection between this engagement with the Coast Survey and the appearance, in July, 1852, in Gould's "Astronomical Journal," of an article by Professor Peirce, entitled, "Criterion for the Rejection of Doubtful Observations." His object was to solve this problem: There being given certain observations, of which the greater part is to be regarded as normal, and subject to the ordinary law of error adopted in the method of least squares, while a smaller unknown portion is abnormal, and subject to some obscure source of error, to ascertain the most probable hypothesis as to the partition of the observations into normal and abnormal. This method or rule given for deciding whether an observation had better be left out of account has received the name, "Peirce's Criterion," and must be regarded as one of his best contributions to science. Tables for use in applying it were soon afterward published by Dr. Gould.

The "Criterion" has been criticised by Professor (now Sir G. B.) Airy as defective in its foundation and illusory in its results; and he was even of opinion that no rule for the exclusion of an observation can be obtained by any process founded purely upon a consideration of the discordance of those observations. This position of the Astronomer Royal must be regarded as entirely untenable; for no observer hesitates to call a widely discordant observation a mistake, and to reject it (when he can find no other reason for so doing), simply because of that discordance. What the mind thus instinctively does, there must be basis at least for a rule for doing. Professor Airy's objections were answered by Professor Winlock at the time of their appearance. The "Criterion" has been used considerably in this country, though not, perhaps, in Europe. The uniform testimony of our computers is, we believe, that it has given excellent discrimination, and that it does not come into conflict with proper judgment based upon experience. This shows the good working of it in actual practice.

That the "Criterion" has not come into use in Europe may

in some degree have been due to the excessive brevity of the argument by which Professor Peirce established the equations to be used. Perhaps no one has read that argument for the first time without finding difficulty in understanding some parts of the reasoning. A want of confidence may thus have easily resulted. Professor Chauvenet has given us a simpler rule for use in rejecting a single divergent observation; but it is only an approximate solution, since one important element is left out of account. Computers need some such rule to guide them, and it would seem almost certain that "Peirce's Criterion," or possibly some modified form of it, will in time secure general acceptance. In any case, it will ever stand as the first, and as a satisfactory, solution of this delicate and practically important problem of probability. At present it is the only solution we believe that claims to be complete.

After the death of Professor Bache, Professor Peirce was, in 1867, made Superintendent of the United States Coast Survey, and he discharged the duties of that office for the next seven years. Soon after his appointment he made a tour of inspection among the parties at work in the field. Notwithstanding his previous intimate relations with the survey as adviser to Professor Bache, he was very much surprised and delighted with the practical skill which many of the officers had acquired. "I recognize at once," he said, "the masters of the profession." Unfortunately, he recognized also the awkward and inefficient, and the presence of these, which even the admirable executive abilities of his predecessor had not been able to eliminate, gave him great concern. Yet he determined to hold to the broadest line of policy, and introduce no rigid discipline that might damp the ardor and spontaneity of the faithful. "The lame and the lazy are always provided for," says the adage; and in the public service they are found, practically, to have the most friends from without, because needing them most. In a scientific service like the Coast Survey, which, unlike many of the departments of the civil service, furnishes absolute criteria from which to judge the merits of an officer, the task of discrimination, if undertaken by a superintendent well versed in the mathematics and physics underlying the manœuvres of the surveyor, would seem to be as easy as it is just. But it was a saying of Professor Bache, that "it would be easy enough to crush directly the men who betrayed the good repute of the service if it was not for uncles, aunts, and cousins, who proposed, in their turn, to crush him."

It was after his return from one of his earliest tours of inspection that Professor Peirce, in conversation with one of the older assistants, said he proposed to give, at least at the outset, greater freedom of action to the officers of the corps, that each

might indicate the full scope of his powers and receive promotion, or give place to another according as the results of his work might determine. "The office," he said, "can add nothing to my reputation unless I can give it greater dignity by raising the standard of the service. I mean to bring the best men to the front and secure publicity to their merits, that they may feel directly responsible to the community and do their utmost for its approbation. To become the leader of a corps of distinguished men is the best thing I can do for the country, for the men themselves, and for my own reputation." This was the policy which he initiated in the Coast Survey, and its wisdom was demonstrated at once. A very large proportion of the officers appreciated his motives, caught the enthusiasm of his genius, and found a new delight in serving a master who coveted nothing, but with rare simplicity lent his own strength to secure to them the full rewards of their labors.

The most important work started by Professor Peirce, and much advanced under his direction, was the actual extension of geodetic work into the interior of the country by continuing the great diagonal arc from the vicinity of Washington to the southward and westward along the Blue Ridge, eventually to reach the Gulf of Mexico near Mobile. He also planned the important work, now in active progress, for measuring the arc of the parallel of thirty-nine degrees, to join the Atlantic and Pacific systems of triangulation, and for determining geographical positions in States having geological or topographical surveys in progress.

He conferred a very important benefit on public interests by so enlarging the scope of the Survey as practically to extend geodetic work into the interior States.

As soon after the war as vessels and officers could be had, he renewed operations for deep-sea soundings and dredgings, and he gave earnest support and aid to all scientific work in any way related to the Survey.

While Superintendent he also took personal charge of the American expedition to Sicily, to observe the eclipse of the sun in December, 1870.

By virtue of his office he was a member of the Transit of Venus Commission, and by his suggestions and active effort he greatly aided that undertaking. Two parties from the Coast Survey were sent out by him,—one to Nagasaki, and the other to Chatham Island, to take part in the work.

The "Quaternion Analysis" of Hamilton seemed to Professor Peirce to promise a very fruitful future. "I wish I was young again," he said, "that I might get such power in using it as only a young man can get." He took great pains to interest his students in it, and in his later years formed a class for its earnest

practical study, with good results. His own thought was turned especially to the logic that underlies all similar systems, and to the limits and the extensions of fundamental processes in mathematics.

At the first session of the National Academy of Sciences, in 1864, he read a paper on the elements of the mathematical theory of quality. Between 1866 and 1870 various papers were read to that Academy, or to this Academy, on "Linear Algebra," "Algebras," "Limitations and Conditions of Associated Linear Algebras," "Quadruple Linear Associative Algebra," etc. These papers were not printed in form as read, but instead in 1870–71 appeared his "Linear Associative Algebra."

His own feeling about this contribution to science is expressed in the salutatory to his friends: "This work has been the pleasantest mathematical effort of my life. In no other have I seemed to myself to have received so full a reward for my mental labor in the novelty and breadth of the results."

An analysis of this treatise was given by Doctor Spottiswoode to the London Mathematical Society, which is characterized by Professor Peirce as "fine, generous and complete." Such an analysis can only come from one who has made a special study of the laws of mathematical thought. To some mathematicians, and other men of science, it may yet be a question, if the time has come for them to say with entire certainty whether this work is to share the fate of Plato's barren speculations about numbers, or to become the solid basis of a wide extension of the laws of our thinking. Those who have thought most on the course which contemporary mathematical science is taking will probably agree that the new ground thus broken can hardly fail to bring forth precious fruit in the future by adding to the powers of mathematics as an instrument.

In any case, the Associative Algebra can never lose its value as an important and most beautiful addition to Ideal Mathematics, and must ever remain a monument to the comprehensive grasp of thought and analytical genius of its author.

Professor Peirce defines mathematics as the science which draws necessary conclusions. Algebra is formal mathematics. Addition is taken to express a mixture, or mere union of elements, independently of any mutual action which might arise if they were to be mixed in reality. From this definition, the commutative character of addition necessarily follows. Multiplication is no further defined than as an operation distributive with reference to addition; but the only algebras treated are those whose multiplication is associative. The subject is further limited to linear algebras, that is, to such as contain only a finite number of lineally independent expres-

sions; so that every quantity considered may be put under the form,

$$ai + bj + ck + \text{etc.}$$

where i, j, k, are peculiar units, limited in number; while a, b, c, are scalars,—a term borrowed from the language of quaternions, but here used in a modified sense to include, not merely the reals, but also the imaginaries, of ordinary algebra. A variety of highly general theorems are given, extending to all linear associative algebras. The author next introduces the conception of a pure algebra, as contradistinguished from one which is virtually equivalent to a combination of several. Methods are developed for finding all such pure algebras of any order. Finally, he obtains the complete series of multiplication tables of these algebras up to the fifth order, together with the most important class of the sixth order. They are in number as follows:

Single Algebras	2
Double "	3
Triple "	5
Quadruple "	18
Quintuple "	70
Sextuple "	65

Professor Peirce never made any extended study of the possible applications of his algebras; he was far from thinking, however, that their utility was dependent upon finding interpretations for them; on the contrary, he showed that certain of them could be advantageously employed, without any interpretation, in the treatment of partial differential equations like that of Laplace.

He read to this Academy in May, 1875, a memoir "On the Uses and Transformation of Linear Algebra," which is, we believe, his only published addition to the principal treatise. He had also made some progress in the investigation of the laws of *non-associative* algebras.

Professor Peirce could not fail to be interested in all questions that concern the equilibrium, the history, and the development of the solar system. At first he was loth to accept the nebular hypothesis in any form. But the results of his studies led him, at last, to defend its main propositions as the true laws of creation.

The rings of Saturn are of prime import in any explanation of planetary development. The discovery by Professor Bond, in 1850, of the dusky ring, and his announcement of reasons for believing that the rings were fluid, multiple, and variable in number, led Professor Peirce to take up the mathematical theory of the rings. He announced, as the result of his analysis, that the rings could not be solid, that a fluid ring could not

have its centre of gravity controlled by its primary, and that it must be supported by the satellites. The principles of the solution were indicated in an article, published in "Gould's Astronomical Journal" in 1851. At different times in the following years some portions of his theoretical treatment of the problem were published. The mathematical possibility of a large number of narrow solid rings was admitted. In the "Memoirs of the National Academy of Sciences" he published, in 1866, the formulas for the potentials and attractions of a ring. This problem has peculiar interest, from the mode of development of the formulas.

The place of comets in the solar system was a subject of his thought even earlier than the rings of Saturn. The discussions and the computation of orbits of various comets in the years 1846–1849, were followed in the latter year by an argument that the comets must have always been parts of the solar system.

In 1859 he applied the theory of solar repulsion of the matter of the comets' tails to the observed form of the tail of Donati's comet, and deduced the strength of the repulsive forces that drove off the nebulous matter. The next year he gave, in a letter to the Academy of Sciences, of Paris, twelve remarkable and suggestive theses on the physical constitution of comets.

In 1861 he made a communication to this Academy, suggesting the meteors as a cause of the acceleration of the moon's mean motion. The paper was not printed, and it does not appear whether he referred to the direct impact of the meteors upon the moon, or to the resistance due to the action of the moon in turning the meteors out of their paths. Probably he included both causes, since each has the effect, to a limited degree, of a resisting medium.

In the last two years of his life he presented to this Academy several communications upon the internal structure of the earth, and the meteoric constitution of the universe. Especially in October, 1879, he gave a series of eight propositions in Cosmical Physics. At an informal scientific meeting at Harvard University he stated five others, which have been since printed in the Appendix to his "Lectures on Ideality in Science." They were given rather as a basis for criticism and discussion than as fully proved. They are founded upon the theory of Mayer, which is advocated by Sir William Thomson, that solar heat, and in part planetary heat, are supplied by the collision of meteors with the sun and planets. Small portions of matter in space cool and become invisible solid meteors. These, by their impact with the sun, produce the violent commotions of the sun's surface. A portion of the earth's heat comes from

the sun, another portion directly from the impact of meteors with the earth's atmosphere. The two portions, he afterwards shows, are equal.

These views are developed more fully in his "Lectures," recently published. The meteors, as Professor Peirce believed, come from the outer portions of the condensing solar nebula. In the course of development an outer shell was left, which furnished the matter to be collected in small masses. The smallest become meteors, the larger comets. Their numbers are enormously great. Arranged according to perihelion distances, the number of comets or meteors coming within a given distance of the sun varies directly as the distance. The heat of Jupiter and Saturn comes from the collisions with those planets. The interior of the earth may be liquid throughout, and the limits set to the lengths of the geologic ages may reasonably be greatly extended.

Any attempt to outline the history of the solar system is sure to lead, in the present state of knowledge, into serious difficulties. Necessarily the problems that arise do not, in many cases, admit of quantitative analysis. The number of unknown elements that appear with every new hypothesis is large; and the more we learn, the larger the number of questions which we cannot answer. It will be but natural if some of the theses of Professor Peirce shall be questioned, and even be proved unsound; but scholars who shall be led into this fascinating field of study will always find in them profound and most suggestive views of creation. Some of these theses will undoubtedly be found to be the true and previously unknown laws of nature.

Professor Peirce was always warmly interested in everything that promoted science in this country. He was generous in his estimate of merit, especially of merit in young men. He was one of the founders of the National Academy of Sciences, was an early President of the American Association for the Advancement of Science, was one of the most active members of this Academy, and was a frequent recipient of academic honors. American science mourns in his death the loss it cannot express, but has a higher life for his having lived.

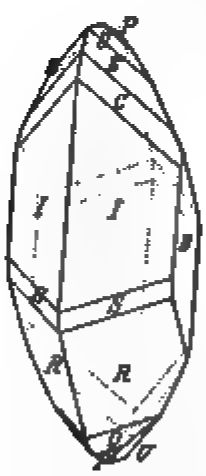
H. A. N.

ART. XXX.—*On the Emerald-green Spodumene from Alexander County, North Carolina;* by EDWARD S. DANA.

THE composition and method of occurrence of the beautiful emerald-green spodumene from Alexander County, North Carolina, was described by Dr. J. Lawrence Smith in a recent number of this Journal;*—the variety was called by him hiddenite after Mr. W. E. Hidden. Dr. Smith's article included a few notes by the writer in regard to the crystalline form of the mineral. The material available for study at that time was scanty and not suited for any accurate determinations of the form. Since then Mr. Hidden has had the kindness to place in my hands a considerable number of crystals, some of them showing the terminations with tolerable distinctness.

The crystals have uniformly a prismatic form, and vary from half an inch to two or three inches in length. They are usually very slender, though sometimes attaining a thickness of one-third to one-half an inch in the direction of the clinodiagonal axis; in the other transverse direction they are much thinner. The crystals show a considerable variety in habit as

1.



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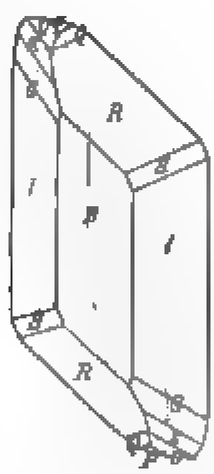

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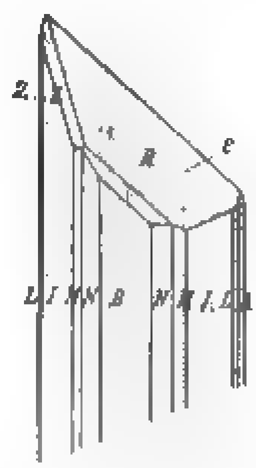

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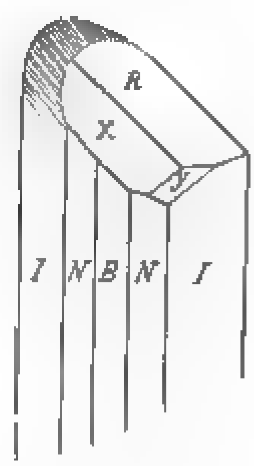


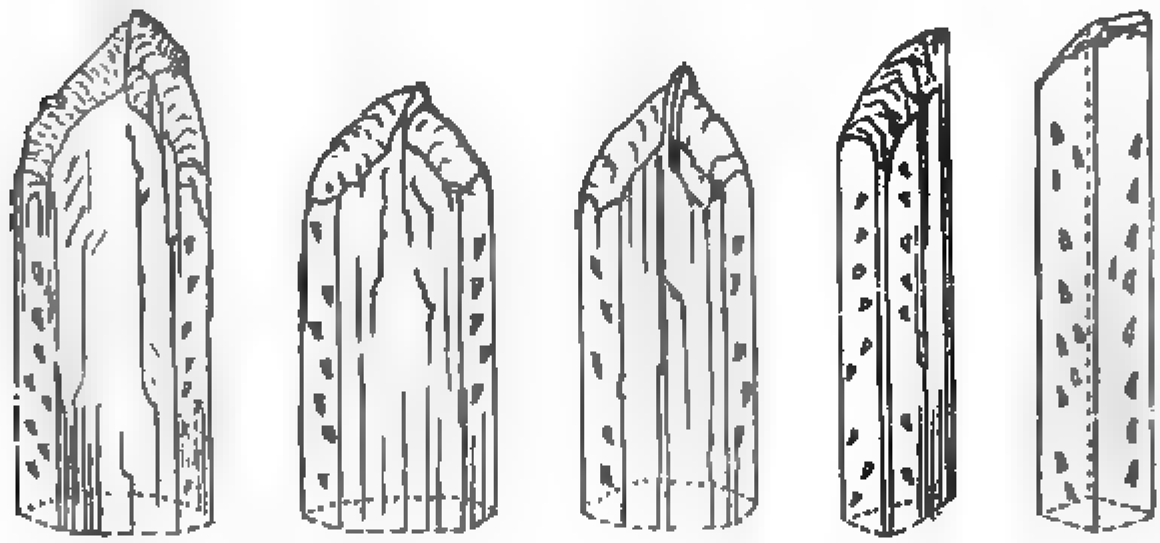
will be inferred from the annexed figures.† Figures 1 and 2 represent the same form but the position of the axes is changed: in fig. 1 the clinodiagonal axis $\grave{a}$ is, as usual, inclined to the front, while in fig. 2 and in the other figures this axis is inclined to the left side, and the orthodiagonal axis b projects to the front. The following figures, 5, 6, 7, 8, 9, are from sketches by Mr. Hidden.

The prismatic planes are uniformly striated vertically, and the crystals are not unfrequently rounded by the oscillatory

* Vol. xxi, p. 128, Feb., 1881.

† The engraver, by mistake, has put the lettering of the cuts in capitals instead of small letters.

combination of the occurring planes in this zone; in addition these planes, more especially those of the fundamental prism *I*, are usually pitted with little depressions which will be more particularly mentioned later. The crystals are often flattened



5. 6. 7. 8. 9.

parallel to the clinodiagonal axis but nearly square forms showing only the prism *I* are occasionally observed. The terminal planes, when they may be said to exist at all, for the crystals are usually terminated very irregularly, are always rough, or striated. The only one of the terminal planes which is at all constant in occurrence is the hemi-pyramid *r* ($\bar{2}21$). The planes *g* (681), *e* (241), *u* ($\bar{2}43$), *p* ($\bar{1}11$) form an oblique zone, as shown in figures 1 and 2, and in the majority of the crystals the presence of the same zone is manifest, although no distinct planes are to be determined, the planes rounding uninterruptedly into each other and continuing the front edge (I/I) over the top of the crystals. This feature is shown in fig. 4 and also in figs. 5, 6, 7 which represent twin crystals.

The twin crystals are common, probably more so than the simple crystals. The plane *a* (100) is uniformly the twinning plane and the twinning-axis is normal to it; it is also the composition plane. The twin crystals are usually nearly symmetrical in form (see figs. 5, 6, 7), and the two halves are united in a sharp well-defined line, as proved by an examination with the polariscope. In the case of crystals not terminated, or with terminations too rough to show whether or not they are twins, the composite character is proved by the little depressions on the planes of the prism *I*, since they are inclined in the same direction both in front and behind (figs. 5 to 9).

The observed planes are as follows:—

a	100	i-i	*q*	$\bar{3}32$	$\frac{3}{2}$
b	010	i-$\breve{i}$	*p*	$\bar{1}11$	1
c	001	*O*	*z*	261	-6-$\breve{3}$
l	320	i-$\frac{3}{2}$	*g*	681	-8-$\frac{\breve{4}}{3}$
I	110	*I*	*e*	241	-4-2
m	120	i-$\breve{2}$	*u*	$\bar{2}43$	$\frac{4}{3}$-2
n	130	i-$\breve{3}$	*ε*	$\bar{2}41$	4-2
s	$\bar{4}41$	4	*x*	$\bar{2}31$	3-$\frac{\breve{3}}{2}$
r	$\bar{2}21$	2	*y*	$\bar{5}61$	6-$\frac{\breve{6}}{5}$

Of the above planes *b*, *s*, *q*, *z*, *g*, *e*, *u*, *ε*, *x*, *y*, are new to the species.

Unfortunately, the crystals, while uniformly perfectly transparent, do not in any case allow of even tolerable measurements, so that no more exact values of the fundamental angles could be obtained than those measured by Professor J. D. Dana with the hand goniometer on the large crystals from Norwich, Mass. On this account these angles are accepted as the basis of calculation, viz:

$c \wedge a$ $001 \wedge 100 = 69° \ 40'$
$I \wedge I$ $110 \wedge 1\bar{1}0 = 93°$
$c \wedge e$ $001 \wedge 021 = 50°$

The corresponding values of the axes are:—

c (vert.)$=0{\cdot}565$ $\quad \bar{b}=0{\cdot}890 \quad \breve{a}=1{\cdot}000$

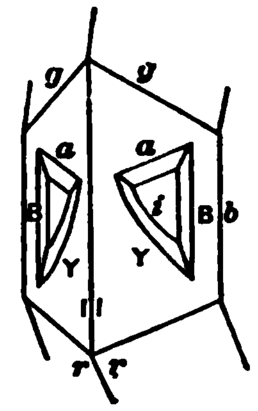
Some of the more important angles (supplement angles) for the occurring planes, calculated from the above axes, are as follows:—

b (010) ∧ *I*	(110)=43°	30′	*b* (010) ∧ *u*	($\bar{2}43$)=40°	16′
" ∧ *l*	(320)=54	55	" ∧ *y*	($\bar{5}61$)=53	27
" ∧ *m*	(120)=25	23	*I* (110) ∧ *g*	(681)=10	18
" ∧ *n*	(130)=17	33	" ∧ *e*	(241)=21	46
" ∧ *g*	(681)=37	3	" ∧ *u*	($\bar{2}43$)=63	8
" ∧ *e*	(241)=36	18	" ∧ *p*	($\bar{1}11$)=75	34
" ∧ *z*	(261)=26	5	*I* ($\bar{1}10$) ∧ *s*	($\bar{4}41$)=17	41
" ∧ *s*	($\bar{4}41$)=41	48	" *r*	($\bar{2}21$)=34	40
" ∧ *r*	($\bar{2}21$)=45	43	" *q*	($\bar{3}32$)=44	22
" ∧ *q*	($\bar{3}32$)=49	57	" *p*	($\bar{1}11$)=59	3
" ∧ *p*	($\bar{1}11$)=58	15	" *y*	($\bar{5}61$)=14	54
" ∧ *x*	($\bar{2}31$)=34	21	" *x*	($\bar{2}31$)=32	14
" ∧ *ε*	($\bar{2}41$)=27	9	" *ε*	($\bar{2}41$)=32	35

As has been stated the measured angles are only rough approximations, they serve however to determine the several planes. As far as needed for this end, in conjunction with the obvious zonal relations, they are as follows:—

I ($\bar{1}10$) ∧ *s*	($\bar{4}41$)=18°	*I* (110) ∧ *g*	(681)=10°
r	($\bar{2}21$)=35	*e*	(241)=22
q	($\bar{3}32$)=45	*u*	($\bar{2}43$)=62
p	($\bar{1}11$)=60	*p*	($\bar{1}11$)=75
y	($\bar{5}61$)=14–15°	*b* (010) ∧ *ε*	($\bar{2}41$)=27
x	($\bar{2}31$)=32	*x*	($\bar{2}31$)=34–34° 30′
		r	($\bar{2}21$)=45–46°

The little depressions observed on the planes in the prismatic zone are an interesting feature of the crystals; those which occur on the planes *I* are the most marked. They appear on the cleavage as well as the natural planes, and often in such numbers as to completely cover the whole surface. Their outline is wedge-shaped and on the front planes (fig. 1) they are inclined upward toward the edge I/I, and similarly downward toward the edge behind in the simple crystals. The form of these depressions is more exactly shown in fig. 10, representing two in symmetrical position and much enlarged. The lower surface is formed by the plane *I*, and the sides by the planes α, β, γ. Of these planes γ is apparently identical with *g* (681), although, as indicated, it is irregular in its intersection with the prismatic plane being curved; α is a plane in the prismatic zone, with $\alpha \wedge I = 5°$, corresponding to the plane $i\text{-}\frac{6}{5}$ or (650) for which the required angle is 5° 13′. The third plane β is in the zone *I*, *g*, *r*, *p*, etc., or that of the unit pyramids. The measured angle of $\beta \wedge I$ is 4°–4° 30′ and this corresponds to the plane $\overline{16}\cdot 16\cdot 1$ (required $\overline{16}\cdot 16\cdot 1 \wedge I = 4°18′$). The plane α is sometimes rounded so as to give an oblique intersection with *I*. The depressions on the plane *b* are also common though less conspicuous than those just named. They are rhomboidal in shape and the outlines are respectively parallel to the prismatic edge, and to the edge b/r.



10.

An examination of a section in the polariscope showed that the bisectrices lie in the plane of symmetry, and that the acute bisectrix (positive) is inclined to the front (fig. 1) edge of I/I at an angle of 26°. These determinations agree exactly with the results given by DesCloizeaux (Mineralogy, p. 351, 1862). A suitable section for measuring the optic axes has not as yet been obtained, one which promised to be satisfactory went to pieces in the hands of the lapidary owing to the highly perfect cleavage parallel to the prism *I*.

It is a matter of some mineralogical interest to note that this variety of spodumene has already found a place among the highly valued gems. The color of the finest crystals is a deep emerald green, and when suitably cut the stones are very beautiful; owing to the dichroism there is a peculiar fire to them which is wanting in the true emerald. The largest stone cut thus far weighs very nearly 2½ carats. Explorations are now being carried on at the locality under the direction of Mr. Hidden.

ART. XXXI.—*The Objects and Interpretation of Soil Analyses;* by E. W. HILGARD, Professor of Agriculture at the University of California.

THE claim of soil analysis to practical utility has always been rested on the general supposition that, "*other things being equal,—productiveness is, or should be, sensibly proportional to the amount of available plant food within reach of the roots during the period of the plants' development;*" *provided*, of course, that such supply does not exceed the maximum of that which the plant can utilize, when the surplus simply remains inert.

The above statement has been, either tacitly or expressly, admitted as a maxim by those who have attempted to interpret soil analyses at all; it being thoroughly in accordance with the accumulated experience of agriculturists, and with their cry for "enough manure" that has been so potent a factor in the development of agricultural science, and of rational agriculture itself. Its acceptance is implied in the search for the solvent that shall represent correctly the action of the plant itself on the soil ingredients; and I shall take it for granted in this discussion, while strongly emphasizing the proviso, especially with reference to physical conditions.

Methods of Soil investigation.—It is universally admitted that the *ultimate* analysis of soils affords little or no clew to their agricultural value; such agents as fluohydric acid and alkaline carbonates go by far deeper than the solvents, naturally acting in soils bearing vegetation, will go within the limits of time in which we are interested.

Many attempts have been made to find solvents whose action on soils would so nearly represent the agents subservient to the needs of vegetation, that conclusions as to the present agricultural value of a given soil could be deduced therefrom. It is needless to recite the long list of such solvents, suggested since soil analysis attracted attention. From fluohydric acid to water charged with carbonic acid, the acid solvents have all signally failed to secure even an approximation to the result desired, viz: a consistent agreement between the quantitative determinations, or the percentages of plant food, found in the several soils, and the actual experience of those who cultivate them.

It has been attempted by the German experiment stations, under Wolff's initiative, to gain an approximation to the relative availability of parts of the soils' store of plant food, by consecutive extractions with acid solvents of different strength, beginning with distilled water and ending with boiling oil of

vitriol or fluohydric acid. I cannot wonder that this laborious process, with solvents arbitrarily chosen, and without any known relation to the solvent action exerted by roots, should have found so little acceptance, and has on the contrary perhaps rather served to confirm the common impression of the uselessness of soil analysis; especially when contrasted with such a huge amount of work, ending after all in mere guesses. I have vainly sought, in the recorded results of such investigations, for any such ray of light on the functions of the several soil ingredients, as would even remotely justify the labor involved.

Causes of failure.—I think there have been two chief factors that have contributed to bringing soil analysis into disrepute in Europe; one is, the fact that virgin soils are there practically non-existent, nearly all the soils analyzed having been at some time subjected to cultivation and concurrently, to the use of manures, thus veiling their original characteristics, and rendering extremely difficult, to say the least, the taking of any sample of soil that shall correctly represent the whole of a large field or district. The second is, the absence of systematic investigation of the subject, since the time of the introduction of the most essential improvements in the determination of some of the chiefly important mineral soil ingredients.

Advantages and need of Soil investigation in the United States.—It is our special and exceptional privilege, that we are still able to secure specimens of the soils of by far the greater portion of the United States, that even the plow has never yet touched, and where manure, outside of the flower and vegetable garden, is an unknown quantity. We can find on these soils their original vegetation, which is so largely used by the settler as a means of diagnosing the actual productiveness of the land he proposes to clear, and of prognosing its durability. The value of this method is so emphatically recognized as to have given rise to the remark, by a distinguished member of this body, that he "would rather trust an old farmer to tell him about the value of a soil, than the best chemist alive."

Now, we may perhaps agree with Professor Johnson in this matter, so long as we find the old farmer on his native heath, and so long as he is exceptionally intelligent. But all farmers are not old; and it is particularly the young ones that stand in need of advice, when they "go west." Moreover, old farmers will frequently disagree widely in their estimate of the qualities and value of a soil; and then who shall decide? And who shall tell the hundreds of thousands of settlers and emigrants annually occupying new lands of whose quality, at present, no one knows anything, what they may reasonably expect of their soil, apart from the bare assertions of inter-

ested parties? How shall they know, in the absence of the old farmer, whether in establishing their homestead in a given locality, they do so for weal or woe, and in which direction they are most likely to secure the highest returns and the longest duration of fertility; and in which direction the first effects of soil-exhaustion will make themselves felt, and how they can best be countervailed?

If the agricultural chemist can do nothing to help the farmer in these important questions, his practical utility will be limited indeed. And how is he ever to be able to render these services, if he continues to ignore the chemical examination of the soils, upon the strength of the "non possumus" pronounced by some high priests?

I cannot consider the *testimonium paupertatis*, implied in the remark above referred to, as well founded. If the old farmer can train his judgment in this matter so as to make shrewd guesses, the agricultural chemist ought to be able to do a great deal better; for he should know all that the farmer does, and a great deal more besides; and, in addition, he should bring to bear on the whole subject a well-trained mind, accustomed to accurate observation and logical reasoning; unlike the old farmer who "knows" that "wheat turns into cheat" in unfavorable seasons.

The chemist who does no more than to give the farmer a column of figures summing up to one hundred or nearly so, opposite another column of unintelligible names, acts simply as an analytical machine; and even to the best of such machines, Professor Johnson's remark will most truly apply. Their enunciations are as enigmatical as those of the Delphic oracle, and as little useful to the farmer as the most accurate analytical formula for calculating the motion and friction of water in pipes would be to the hydraulic miner who stands at the nozzle of the "monitor." Both the miner and the farmer might be greatly benefited by the information conveyed, if they could only understand it.

Since, then, the figures of a soil analysis, no matter how made, do not interpret themselves, by what rule or rules shall we be governed in interpreting them for practical purposes?

Of the older attempts in this direction, it is scarcely necessary to speak. What remains of them at this time, may be briefly summed up in the statement, that it is usual to judge a soil by its absolute percentages of plant-food on the one hand, and by such scanty information as we can elicit regarding their availability, on the other. As to what constitutes "much" or "little" or "a deficiency" of any one ingredient, doctors differ as widely as in respect to the classification of soils. It has been usual to take a notoriously very rich soil as a type, and com-

pare others therewith; but even a cursory comparison shows that, in many cases, soils showing percentages of plant-food very much inferior to those of the type are nevertheless in practice found quite as productive; and that even in cases where precisely the same solvents had been used in their extraction. These facts are too well known to require exemplification; and they led to the exclusive adoption, in the study of the part played by the several soil ingredients, of the methods of culture on artificial soils or in solutions of known composition.

The radical fault of these methods is that they necessarily deal with plants placed under artificial conditions, and with mediums of nutrition whose comparison with natural soils is at best a lame one; necessarily so, until we shall know much more than we do of the intimate condition and functions of the soil as a whole, and of its ingredients, both severally and jointly. And while the artificial cultures have given us some exceedingly valuable information as to the relative importance of certain soil ingredients, it is still held by some of the highest agricultural authorities, that the only way to obtain practically useful data as to the best method of soil improvement in any particular case, is to *go and try*—first on the small, and then on the large scale; and when a particular kind of manure finally fails of effect, to go and try again; and so on.

Are we then really reduced to such empiricism as this—are the permutations and combinations of nitrogenous, phosphate and potash manures, all that agricultural chemistry can do for the western farmer, when his "inexhaustible" soil begins to be "tired?"

System of investigation adopted.—Unwilling to abide by this lame solution of the problem, I have endeavored to solve it, or at least to approach its solution, from a somewhat different side, as suggested by the opportunities offered in the agricultural surveys of the newer States. Taking for granted the soundness of the old farmer's judgment of the productiveness of a soil from its natural vegetation, I have sought to determine, by close chemical and physical examination of the soils in their natural condition, the causes that determine this natural selection on the part of certain species of trees and herbaceous plants; while at the same time observing closely the behavior of such soils under cultivation, their special adaptations, etc. It goes without saying that this can be done most successfully where, as in the Western and Southern States, virgin soils are still obtainable, where manure is unknown, and where the simple history of each field can easily be gathered from the lips of the settler who first broke the sod.

It is evident that when used in this connection, and made uniformly and systematically, with a definite problem in view,

each soil analysis becomes an equation of condition; and that by the proper treatment of a large number of such, by a logical process of elimination, the problem of the function and value of each soil-ingredient or soil-condition can be approached with a better prospect of a solution in accordance with *natural* conditions, than can be expected from cultures upon artificial soils, or in solutions.

My first trials of the efficacy of this method of investigation were made upon the soils of the State of Mississippi, which, fortunately, present extreme variations in character in almost every direction, and upon every key, so to speak, of the soil scale. But for this fact, I might, like many before me, have abandoned in despair, the hope of attaining any definite results. Some of the conclusions reached in this work have been given in previous papers (this Journal, Dec., 1872, and others). Since then, the material has been considerably increased, and quite lately, the investigations made under the auspices of the census office, upon the soils of the cotton States, have greatly added thereto, and given a wider scope to the comparisons. The detailed record and discussion of the facts so gathered will form part of the Census report on cotton culture, and in any case would be far too voluminous for presentation here. I must therefore confine myself to indicating, in general, some of the main points involved.

The taking of representative soil specimens is, of course, a matter of first importance, and sometimes of no little difficulty. All those analyzed under my direction have been taken in accordance with printed directions, with care in the selection of proper localities, the discrimination between soil and subsoil, a record of depth, natural vegetation, behavior in cultivation, etc. As heretofore stated, I find that with such care, it is perfectly practicable to obtain samples representing, typically, soil areas of many thousands of square miles; especially so when the subsoils are taken as the more reliable indices.

Method of Analysis.—In the selection of the *solvent for making the soil-extract* to be analyzed, I have been guided by the consideration, that minerals not sensibly attacked by several days' hot digestion with strong hydrochloric acid, are not likely to furnish anything of importance to *agriculture*, within a generation or two. If this assumption seems arbitrary, it at least commends itself to common sense. The heavy draught made upon the soil by the removal of crops cannot be sensibly effected by the minute additions made to the available plant food by the atmospheric or root action on such refractory minerals.

Regarding the *strength of acid* to be used, and the *time* necessary to secure the solution of the important substances, I have

caused investigations to be made by Dr. R. H. Loughridge (this Journal, Jan., 1874, p. 20), on a subsoil selected for its representative position and derivation—a drift soil covering, probably, some 15,000 square miles in the uplands of Western Tennessee and Mississippi, and perhaps as fully "generalized" in its origin as can be obtained. The result of this investigation was that hydrochloric acid of about the specific gravity of 1·115 seems to exert the maximum effect, and that the extraction is practically complete after a water-bath digestion of five days. These conditions of digestion have been substantially maintained in all the investigations made under my direction. An excess of time of digestion results simply in higher percentages of alumina and soluble silica, or what is equivalent, in a farther decomposition of kaolinite particles.

The *methods of analysis* used by me are substantially those given in the first Kentucky Report, volume I, by Dr. Robert Peter, with such changes as the progress of analytical chemistry suggested. All the reagents have been especially prepared, or purified, in the laboratory itself; porcelain beakers only have been used in the digestions; and generally every possible precaution has been taken to insure correctness in the determination of the minute percentages of the important ingredients. Numerous repetitions have, in most cases, confirmed the correctness of the work.

Of other determinations, the one preceding all analytical operations has been the determination of the "moisture-coefficient" of the "fine-earth," by exposing a very thin layer of the same to a *fully* saturated atmosphere for at least twelve hours, at a sensibly constant temperature. As previously stated, I have in these determinations come to results differing materially from those obtained by Knop, Schübler, and others; probably because of the more complete fulfillment of the conditions of full saturation of air as well as soil. I find that for most soils, the absorption-coefficient is practically constant at temperatures between +7° and +25° C.; and contrary to the conclusions reached by Adolph Mayer.

I find that this coefficient exerts an exceedingly obvious and important influence upon the actual productiveness of soils. An investigation reaching beyond the temperature-limits mentioned, and also embracing the use of a partially saturated atmosphere, has just been made in my laboratory and will shortly be published.

A determination of the total "*volatile matter*" of the soil, that is, its organic matter and combined water, by ignition, is made on the portion of soil used for the determination of *phosphoric acid* by means of molybdic acid. While this determination is necessary to the "summing up" of the analytical state-

ment, it is not in itself very instructive, as it leaves the relative amounts of the two substances altogether indefinite. A determination of the organic matter by combustion, or by extraction with potash lye, is also unsatisfactory, because of the impossibility of excluding from these determinations, a large amount of comminuted, but altogether crude and unhumified, vegetable matter; which becomes very obvious under the microscope, or in the process of silt-analysis. I have therefore adopted for the determination of active humus, the admirable method of Grandeau, by the aid of which at least a uniform *minimum* determination becomes possible.

I have not devised any method for the direct determination of the *water of hydration*, although there are cases in which it would be very desirable to have this item, for the determination of the condition of the alumina and ferric oxide.

I have in a few cases determined the amount of *silica soluble* in boiling solution of sodic carbonate in the *crude* soil. But this determination is often beset with almost insuperable mechanical difficulties, from the diffusion of the clay in the alkaline liquid. It does not appear to promise results of sufficient importance to justify such labor; the more, as by the method of Grandeau, the actual available amount of silica can probably be better determined. But I have found the determination of the *silica soluble in the alkaline carbonates, in the "insoluble residue" of the acid extraction*, of very great interest. Evidently, in so far as it is derived from the decomposition of clay, "kaolinite," it should stand in a definite ratio to the alumina dissolved by acid, and this is often very strikingly the case. But sometimes the soluble silica is so entirely out of proportion to (below) the amount required to form kaolinite with the dissolved alumina, as to prove that the latter is present in a different condition: the only possible one in that case being that of hydrate. This fact, doubtless, accounts for a great deal of the otherwise incomprehensible variations in the properties of soils and certain clays, which I shall hereafter discuss. I should also mention in this connection that I have strong evidence of the presence of still another hydrous silicate, related to saponite, in some of the tertiary "prairie soils" of the Southern States; the peculiarities of which, when under cultivation, have seemed unintelligible.

I have not yet been able to extend the method of Grandeau for humus extractions over a sufficient number of widely different soils of well known characteristics, to consider the claim of its furnishing a definite measure of the available plant-food in the soil, as definitely established. But thus far I have found nothing to contradict this probable assumption, and much tending to its confirmation; and I hope to be able to continue the

investigation of its relation to the productiveness of soils, to a definite conclusion. There can be no reasonable doubt that what *is* extracted by Grandeau's ammonia water is at the command of the solvents employed by plants; the only question is, to what extent plants can readily go beyond. This of course requires extended culture experiments, on a great variety of soils.

The determination of the *phosphoric acid* and *silica* in the residues from the ignition of Grandeau's extracts have already furnished most important data concerning the cause of the productiveness of some soils having comparatively a low percentage of phosphates.

As regards the determinations of *nitrogen* and its compounds in the virgin soils thus far analyzed, I have omitted them in part from want of time and proper appliances for these delicate determinations, and partly from a doubt of their usefulness. The constant variation and inter-convertibility of nitrates and ammonia-compounds renders their determination at any given time, of interest for that time only; and as the nitrogen percentage of the mould of natural soils adapted to agriculture is not likely to vary much, the humus-percentage may probably be taken as roughly proportional to the total nitrogen of the soil. A full investigation of this subject is, of course, also called for. On the other hand, I find that the fulfillment of the conditions of nitrification in the soil, is in all cases a condition of its thriftiness.

Interpretation of the analytical results.—Having obtained, as above outlined, the percentage composition of a soil, how are we to interpret these percentages to the farmer? what are "high" and "low" percentages of each ingredient important to the plant, whether as food or through its physical properties?

The first question arising in this connection, is naturally, whether all soils, having what experience proves to be high percentages of plant-food when analyzed by the processes above given, show a high degree of productiveness?

So far as my experience goes, this question can, for virgin soils, be unqualifiedly answered in the affirmative; *provided* only, that improper physical conditions do not interfere with the welfare of the plant.

But it does not therefore follow, as was at first supposed, that the converse is true, and that low percentages necessarily indicate low production. This will be apparent from a simple consideration.

Suppose that we have a heavy alluvial soil of high percentages, and producing a maximum crop in favorable seasons. We may dilute this soil with its own weight, or even more, of coarse sand, thereby reducing the percentages to one-half, or

less; and yet it will not only not produce a smaller crop, but it is more likely to produce the maximum crop every year, on account of improved physical conditions. If we compare the root system of the plants grown in the original, and in the diluted soil, we will find the roots in the latter more fully diffused, longer, and better developed; not confined to the crevices of a hard clay, permeating the entire mass, and evidently having fully as extensive a surface-contact with the fertile soil particles, as was the case in the undiluted soil.

How far may this dilution be carried without detriment?—The answer to this question must largely be experimental and must vary with different plants; which is precisely what the farmers' experience has shown, long since. A plant capable of developing a very large root-surface, can obviously make up by greater spread, for a far greater dilution than one whose root surface is in any case but small. The former flourishes even on "poor, sandy" soils, while the latter succeeds, and is naturally found on "rich, heavy" ones only; although the absolute amount of plant-food taken from the soil may be the same in either case.

Now the conditions here supposed are frequently fulfilled in nature, and more especially so in alluvial soils. Among many striking examples that might be given, are the analyses of two soils about equally esteemed for the production of cotton, both equally durable, so far as experience has gone, and yet differing so in their percentages of mineral plant-food, to the extent of from three to five times.

In cases like these, which are not at all infrequent, the mere percentage of plant-food in the soil showing the low figures, would lead to a most erroneous estimate of its agricultural value. But when, in addition to these, we know the fact that in the one, the food-roots can exercise their functions to the depth of three or four feet, while in the richer soil with ordinary cultivation, they will rarely reach to a greater depth than twelve or fifteen inches, the equal productiveness becomes quite intelligible.

It is obvious, then, that without a knowledge of the respective depths and penetrability of two soils, a comparison of their plant-food percentages will be futile. Nor is it feasible to agree upon a certain depth to which all soils analyzed should be taken. The surface soil with its processes of humification, nitrification, oxidation, carbonic acid solution, etc., in full progress, must always be distinguished from the subsoil in which these processes are but feebly developed, and where the store of plant-food—in which it is generally richer than the surface soil—is comparatively inert. Hence the obvious importance of specimens correctly taken, and the necessity of intelligent and accurate observations on the spot.

I have attempted to make allowance for the cases of dilution, as above noticed, by combining the results of the mechanical with those of chemical analysis. In the investigation made by Dr. Loughridge, of the several sediments obtained in the mechanical analysis of the typical soil above referred to—see this Journal, Jan. 1874—it appeared that plant-food practically ceased to be extracted from sediments exceeding 5^{mm} hydraulic value; and in re-calculating the percentages of soils *of the same general derivation*, after throwing out the coarser sediments, we often find very striking approximations to identity of percentage composition, as well as of proportionately *inter se.* It is obvious, however, that this cannot be generally true; since inert clay or impalpable silt must often come in as diluents. Nevertheless, I consider the mechanical analysis of soils (carried out by the method heretofore described by me, and *not* in accordance with that of the German experiment stations), as an almost indispensable aid in judging fully of the agricultural peculiarities of soils, especially when these cannot be personally examined in the field.

The concentration of the available portion of the plant-food of soils in their finest portions is almost a maxim already, scarcely needing the corroboration afforded by the investigation of Dr. Loughridge above quoted. A "strong" soil is invariably one containing within reach of the plant a large amount of impalpable matter, although the reverse is by no means generally true. Striking corroborations of this maxim are afforded by the steady increase of certain plant-food percentages in the deposits of streams as we descend, and the proverbial richness of "delta" soils is exactly in point.

"High" and "low" percentages and their interpretation.—I will now state, as concisely as possible, some of the main points I consider as substantially proven by the comparisons of soil analyses made upon the uniform plan outlined above. The detailed record upon which these conclusions rest, would render this paper far too long, but will be given in the Census report upon cotton culture.

1. *Other things being equal, the thriftiness* (i. e., present productiveness) *of a soil is measurably dependent upon the presence of a certain minimum percentage of lime.*

The evidence I can present in support of this maxim is overwhelming. It is obvious to the eye in thousands of cases, when the significance of the occurrence of certain trees, esteemed by the "old farmer" as certain signs of a productive soil, is once understood. Almost all the trees he habitually selects as a guide to a good "location," are such as frequent *calcareous* soils, using the term, however, in a somewhat different meaning from that usually given it. That is, I find that in order to manifest itself

unequivocally in the tree-growth, the lime-percentage should not fall below 0·100 in the lightest sandy soils; in clay loams not below a fourth of one per cent, 0·250; and in heavy clay soils, not below 0·500, and may advantageously rise to one and even two per cent. Beyond the latter figure, it seems in no case to act more favorably than a less amount, unless it be mechanically.

The effect produced by the presence of such, or greater percentages of lime in the soils seems to be a kind of "aufschliessung," an energizing or rendering active of that which otherwise would remain inactive. This becomes evident at once in the smaller insoluble residues from the acid treatment, yielded by such soils; there being then oftentimes a complete dissolution of the alumina, a large part of which ordinarily remains behind in the shape of clay (kaolinite-particles). It would seem that as regards the silicates, the carbonate of lime in soils performs in a measure, the same functions as the caustic lime in Lawrence Smith's method of silicate "aufschliessung." We have an indication of the same action in the case of marls, whose small percentages of potash and phosphates act so energetically, and in which we so often find the potash in the highly available form of glauconite grains; also in the displacement of potash from zeolitic compounds, by lime or lime salts.

From the evidence before me, I should specify as follows, the advantages resulting from the presence of an adequate supply of lime in soils:

a. A more rapid transformation of vegetable matter into *active* humus which manifests itself by a dark, or deep black tint of the soil.

b. The retention of such humus, against the oxidizing influences of hot climates; witness the high humus-percentages of such soils, as against all others, in the Southern States.

c. Whether through the medium of this humus, or in a more direct manner, it renders adequate for profitable culture percentages of phosphoric acid and potash so small that, in the case of deficiency or absence of lime, the soil is practically sterile.

d. It tends to secure the proper maintenance of the conditions of nitrification, whereby the inert nitrogen of the soil is rendered available.

e. It exerts a most important physical action on the flocculation, and therefore on the tillability of the soil, as heretofore shown by Schlœsing and by myself.

I may add that in the great majority of soils (excepting those that are extremely sandy) the lime-percentage is greater in the subsoil than in the surface soil. This is, doubtless, the result of the easy solubility of calcic carbonate in the soil water, which carries it downward and thus tends to deplete the sur-

face soil. This fact is strikingly shown in the results of Loughridge's investigation on the composition of the several sediments. (This Journal, January, 1874, p. 19).

The efficacy of lime in preventing "running-to-weed" in fresh soils, and in favoring the production of fruit, is conspicuously shown in a number of cases.

This controlling influence of lime renders its determination, alone, a matter of no small interest; since its deficiency can very generally be cheaply remedied, avoiding the use of more costly fertilizers.

I have been unable to trace any connection of *magnesia* with any of the important qualities of soils. Its percentage is usually larger than that of lime, frequently about double.

2. The *phosphoric acid* percentage is that which, in connection with that of lime, seems to govern most commonly the productiveness of our virgin soils. In any of these, less than five hundredths (0·05) must be regarded as a serious deficiency. In sandy loam soils, one-tenth (0·100), when accompanied by a fair supply of lime, secures fair productiveness for eight to fifteen years; with a deficiency of lime, twice that percentage will only serve for a similar time. The maximum percentage thus far found in an upland soil by my method of analysis, is about a quarter of one per cent (0·250), in the splendid tableland soils of West Tennessee and Mississippi. In the best bottom ("buckshot") soil of the Mississippi, three-tenths (0·30). In that of a black prairie of Texas, 0·46 per cent, this being the highest figure that has come under my observation.

How the lime compounds contained in the soil act in rendering the phosphates more available, I do not pretend to discuss at present. A number (far too limited as yet) of determinations made according to Grandeau's method, appear to confirm the inference that calcareous soils yield to this treatment a larger relative percentage of available phosphoric acid, than those deficient in lime.

3. The *potash-percentages* of soils seem, in a large number of cases, to vary with that of "clay;" that is, in clay soils they are usually high, in sandy soils low; and since subsoils are in all ordinary cases more clayey than surface soils, their potash-percentage is almost invariably higher also. 1·3 per cent K_2O is the highest percentage obtained by my method of extraction, and that from the same soil that afforded the second highest phosphate percentage also, the "buckshot" of the Mississippi bottom, noted for its high and uniform production of cotton. As the same soil contains 1·4 per cent of lime, and is jet black with humus, it may well serve as the type of a fertile soil.

The potash-percentage of heavy clay upland soil and clay loams ranges from about 0·8 to 0·5 per cent, lighter loams from 0·45 to 0·30, sandy loams below 0·3, and sandy soils of great

depth may fall below 0·100 consistently with good productiveness and durability; the former depending upon the amounts of lime and phosphoric acid with which it is associated. Virgin soils falling below 0·060 in their potash-percentage seem, in all cases that have come under my observation, to be deficient in available potash, its application to such soils being followed by an immediate great increase of production.

Since but few soils fall below this minimum, my general inference has been that potash manures are not among the first to be sought for after the soils have become "tired" by exhaustive culture. The universal preference given to phosphatic and nitrogenous fertilizers in the west and south, is in accord with this inference. In the older portions of the United States, "kainite" is becoming more important, while in the alkali lands of California, soluble potash salts often impregnate the soil water.

4. In all soils not specially impregnated with sea or other salts, the amount of *soda* extracted by the acid is considerably *below* that of potash in the same soil, varying mostly from one-eighth to one-third of the percentage of the latter. When much more is found in such soils, a repetition of the determination will usually show that the separation from magnesia was imperfectly made. I can trace no connection between the soda percentage and any important property of the soil, any more than in the case of *magnesia* and *manganese*, albeit none of these is ever absent from ordinary soils.

5. *Sulphuric acid* is found in very small quantities only, even in highly fertile soils. From two to four hundredths of one per cent (0·02 to 0·04), seems to be an adequate supply, but it frequently rises to one-tenth (0·1) per cent, rarely higher.

6. *Chlorine* I have as a rule left undetermined, on account of its constant variability and universal presence in waters, and acknowledged slight importance to useful vegetation.

7. *Iron*, in the shape of ferric hydrate finely diffused, appears to be an important soil ingredient on account of its physical, and partly also its chemical properties. The universal preference given to "red lands" by farmers, is sufficiently indicative of the results of experience in this respect, and I have taken pains to investigate its causes. The high absorptive power of ferric hydrate for gases is probably first among the benefits it confers. Red soils resist drought better than similar soils lacking the ferric hydrate. And here I must again call attention to the strange fallacy in Adolph Mayer's experiments on the wilting of plants in drying soils, from which he deduces as probable, the maxim that the hygroscopic coefficient of soils is a matter of indifference to plants. His plants *in pots* were not under the conditions in which field crops are when called upon to resist drought, whether from drying winds, or hot sun.

Here the continuous rise of moisture from the subsoil tends to keep up the supply to the water roots, while at the same time nutrition, as is well-known, continues almost unabated in air-dry soils, so long as there is no injurious rise of temperature in consequence of that dryness. But that is precisely the point where a high moisture-coefficient comes into play, by preventing, in consequence of evaporation, a rise of temperature that, under similar circumstances would prove fatal to the surface roots of the crop in soils of low absorption power. In fact, Mayer's conclusion is at variance with the ordinary experience of centuries, repeated every day in the droughty regions of the South and of the Pacific coast. It takes more than flower-pot experiments to invalidate the universal designation of soils of low hygroscopic power, as "droughty."

The moisture-coefficient depends in ordinary soils, upon one or more of four substances, viz: (in the order of their efficacy), humus, ferric hydrate, clay and lime. It varies in cultivatable soils from about 1·5 to 23 per cent at 15° C., and in a saturated atmosphere. A pure clay rarely exceeds 12 per cent; ferruginous clays show from 15 to 21; some calcareous clay soils rise nearly as high, while peaty soils rise to 23 per cent and even more, but the efficacy of the ferric hydrate depends essentially upon a state of fine division. When merely incrusting the sand-grains, or aggregated into bog-ore grains, it exerts little or no influence, although the analysis may show a high percentage. Sometimes soils highly colored show but a small iron percentage, while yet, on account of very fine diffusion, the advantages referred to are realized.

From 1·5 to 4·0 are ordinary percentages of ferric oxide, occurring even in soils but little tinted. Ordinary ferruginous loams vary from 3·5 to 7·0, highly colored "red lands" have from 7 to 12 per cent, and occasionally upward to 20 and more.

Of course, a large amount of ferric hydrate facilitates the tillage of heavy clay soils, and its color tends to the absorption of heat. But I incline strongly to the belief that the benefits of its presence are not confined to physical action. From the fact that highly ferruginous soils rarely have a high percentage of humus, it appears that the former acts as a carrier of oxygen to the latter, and thus probably favors, especially, nitrification.

On the other hand, such soils are the first liable to damage from imperfect drainage, overflows, etc. The reduction of the ferric hydrate to ferrous salts, most commonly in the subsoil, manifests itself promptly by the "blighting" of the crop. But under natural conditions this can rarely occur, because a frequent recurrence of conditions favoring reduction will inevitably result in a gradual bleaching of the soil, and an accumulation of its iron in the subsoil in the form of bog-ore or "black pebble."

In bringing forward this hasty summary of the conclusions either definitely justified or foreshadowed by my investigations on the subject of soil composition, I do not, of course, look for their acceptance until the record and proofs shall be forthcoming, as they soon will, in another publication. My present object is to call attention once more to the fundamental and practical importance of the subject of soil examination by all available means, and to protest against the contemptuous, unreasoning putting aside of the whole matter of soil analysis, that has become current in works on agricultural chemistry for some time past. If the chemists of Europe are content to declare themselves incompetent to accomplish anything more than mere guesses by the analysis of their long cultivated and manured soils; if the same should even be held as regards the well-worn soils of New England, the objection cannot be sustained as against the virgin soils of our newer States and Territories, or even as against *any* soils that have not been manured as yet, these two classes constituting, probably, four-fifths of all the cultivatable lands of the United States. These soils have been subjected only to natural, or to definitely ascertainable artificial influences. They are sensibly uniform over very large areas, or at least, vary uniformly; they still possess, in part at least, their original tree or other growth, as produced by natural selection. Is it reasonable that in the presence of such opportunities American chemists should also declare themselves incompetent, without even trying to accomplish that which both in a theoretical and in a practical point of view, cannot be held otherwise than as of prime importance?

No one can be more sensible than I myself, of the small amount of progress made in the matter of *à priori* recognition of the agricultural character and value, present and ultimate, of soils, in the twenty-five years during which I have more or less pursued the study of the subject. It would doubtless have been otherwise had any one besides myself worked in this field of research with similar objects and methods. By the early death of Dr. David Dale Owen, I was deprived of the one through whose initiative and encouragement I first entered upon and persevered in this field, through the discouragement freely bestowed upon me by my fellow-chemists; and thus the excellent work done by Dr. Robert Peter, Dr. Owen's chemical assistant in the survey of Kentucky and Arkansas, in the analysis of the soils of those States, has so far remained without an interpreter. If the facts, suggestions and views here presented should be successful in attracting to this field of research some of the attention now so lavishly bestowed on the investigation of recondite organic compounds, the object of this paper will have been attained.

Art. XXXII.—*Mineralogical Notes*; by B. Silliman.

1. Vanadinite and other Vanadates, Wulfenite, Crocoite, Vauquelinite, etc., from Arizona.

I here record the discovery of two important and very interesting mineral localities, or districts, in the Territory of Arizona, from one of which I have obtained vanadinite of remarkable beauty of color and perfection of crystalline form, associated with almost equally beautiful wulfenite of an orange-red color; and from the other, four, perhaps more than four species containing vanadium. The last named district has also furnished crocoite and vauquelinite never found before, I believe, in North America.

I am greatly indebted to my faithful and intelligent correspondent, Mr. George A. Treadwell, of Vulture, Arizona, for sending me, for some years past, a great number of minerals and ores collected by him in that Territory, among which are those now to be described. I mention also, with pleasure, the aid afforded me by Mr. Edward Farley, of Wickenburg, owner of several interesting veins, and Dr. Jones, of Phœnix. Mr. F. F. Thomas, lately in charge of Silverlead Furnaces, near Silent, in Arizona, and Mr. John McDougal, Superintendent of mines, have also contributed important data in extending our knowledge of that interesting Territory.

Vanadinite.—This hitherto rare species promises now to be comparatively abundant. In the so-called "Silver District," in Yuma County, Arizona, about fifty miles north of Fort Yuma, is a large area traversed by veins of quartz carrying argentiferous galena, with salts of lead, but no gold, and rather extensively explored. The lead salts which I have seen from this region are wulfenite, of remarkable beauty, vanadinite, and massive anglesite with galenite. Vanadinite occurs in three mines, near together, the "Hamburg," the "Princess" and the "Red Cloud." The crystals of vanadinite are extremely beautiful, alike for brilliancy of color, luster and perfection of form. Only a single vanadate appears to occur in the Silver District; but there may be an exception to this remark, since a greenish yellow incrustation on one specimen may turn out to be volborthite or one of the other amorphous vanadium minerals. All the veins of this district occur, as I am informed by Mr. Thomas, between a foot-wall of granite and a hanging-wall of porphyry, specimens of which rocks I have in hand. The foot-wall of granite is somewhat irregular, but the porphyritic hanging-wall is well defined. I have not yet made sections of the latter rock; it closely resembles the augite trachytes of Nevada and elsewhere,

the usual associates of silver ores the world over. All these lodes abound in calcareous matter, but there are no limestone beds in the vicinity; and in the absence of any organic remains, we are ignorant of the probable geological horizon.

"The Hamburg" mine has furnished the most numerous and, on the whole, the best specimens of vanadinite. The crystals vary in color from deep orange-red—deeper than potassium bichromate, but not ruby—through lighter shades of orange-red to reddish-yellow and brown. They are always highly lustrous. The size is small, the length being not over two millimeters and usually less than one; and the diameter about half the length to equal dimensions. The hexagonal prisms are modified usually by one, sometimes by two planes on each terminal edge, and occasionally the angles are replaced. These crystals are implanted singly and in crusts on a dark chocolate-colored siliceous gangue, with occasional obscure crystals of cerussite, and rarely a dark-colored cleavable lime-rock (impure calcite).

The "Red Cloud" mine furnishes vanadinite of a rich orange-red or flame-color, associated with beautiful orange-red wulfenite. At the depth of 280 feet, measured on the slope of the vein, wulfenite takes the place of vanadinite almost to its exclusion. The crystals of vanadinite at this mine are smaller and grouped in more confused masses than at the Hamburg mine.

At "the Princess" mine, the vanadinite occurs in slender crystals of a brilliant red color almost identical with that of crocoite, implanted upon white calcite. The habit of the species is unlike either of those before mentioned; the crystals are at least four diameters long and are very slightly modified. They are not over half a millimeter in diameter, but are very perfect in form, luster and color. They have, naturally enough, been mistaken for chromate of lead.

The genesis of the vanadates of these mines is obscure. A single small specimen only of the galena has reached me. It forms the nucleus of a surrounding mass of amorphous anglesite, upon the outer surface of which appear obscure crusts of vanadinite. Analyses of a series of samples selected on the spot, by a careful examination might reveal the origin of the vanadic acid.

Vulture district, as I have called it, another and quite distinct district in Arizona, has furnished, at a number of places, vanadinite with other rare species. This area embraces the country between the Hassayampa River on the west and Agua Fria on the east, and extends in a north and south direction from the well known Vulture Mine to Antelope Mountains, Weaver district, on the road to Prescott. It is

partly in the lower portion of Yavapai County, and partly in the northern portion of Maricopa County. Within this area are numerous veins of gold-bearing quartz carrying lead and sometimes a little copper or perhaps both at the same time. I have become familiar with the mineralogical character of these veins through Mr. G. A. Treadwell: and Mr. Edward Farley, owner of some of the mines which have furnished the most interesting species of this area, has prepared for me a sketch map on which the localities are laid down with sufficient accuracy for identification by reference to the Government map.

Farley's "Collateral Mine," about twenty miles northeast of Vulture, is perhaps the most interesting locality of vanadinite in this area. The vein is about four and a half feet wide, and occurs in soft gray talcose rock. About one-half of the thickness of the vein, on the hanging wall side, is quartz stained green with chrysocolla, and chocolate-brown with a ground-mass which I find carries vanadium, and showing lemon-yellow stains resembling plumbic ochre, also a vanadate. Any portion of these yellow and brown masses (if pulverized, digested with dilute nitric acid, and the filtrate treated with ether) gives a strong reaction for vanadic acid. Unless led to test this gangue-stone for vanadium by the occurrence of vanadinite in other parts of the vein, no suspicion of its presence would be aroused. A seam of very red ferric oxide with calcite follows next, and the red oxide of iron reacts decidedly for vanadium, while the calcite is penetrated with yellowish and white fibrous crystals of vanadinite. Next, there is a seam, of about six inches, of very soft material filled with abundant lemon-yellow acicular crystals of vanadinite in tufts and aggregated masses, the whole quite friable, forming the center of the vein, which also carries in this zone masses of cerusite. The whole mass of this soft material reacts very strongly for vanadic acid. Then follow, on the foot wall, about twenty inches of vein-matter composed of quartz with calcite. The calcite is penetrated with acicular crystals of vanadinite arranged in threads and in stellar tufts, usually not over a line in thickness, but occasionally opening into small cavities, like geodes, lined with distinct hexagonal prisms of this species. The common color is yellow but they are often nearly white. The cleavage fragments of calcite carrying the vanadium crystals form specimens of rare beauty. Quartz, similar to that in the upper section of the vein, is found in this lower member, and this is also somewhat stained with copper silicates. In its open joints occur hexagonal crystals of vanadinite of a fine yellow color; they closely resembling mimetite, but give no arsenical reaction, and I failed to obtain

a trace of arsenic from the included crystals in the calcite, when tested by soda and potassium cyanide in the closed tube. This quartz carries crystals of vanadinite in habit very unlike those which occur at the Hamburg mine in the Yuma district; they are long slender needles hardly a line in thickness, of a delicate straw-yellow color, quite transparent. They are associated with others of a rich orange-yellow color and not so well defined. There are also confused tufts of crystals of the same species, not thicker than hairs, of a pure chrome-yellow color, implanted in cavities in the red-iron-stained gangue.

Descloizite (?)—A mineral which may prove to be descloizite, occurs among the ores of the Collateral Mine which have reached me. It is found in blue-black and brownish-black semi-transparent and very brilliant crusts, the individuals imperfectly developed; hardness about 3—3·5; streak-yellow to brownish-yellow. Alone in the closed tube it fuses and gives off abundant water. It reacts very strongly for vanadium and for lead, also for copper, manganese and zinc. Since this paper was in hand I have received from Mr. Farley, under date of June 25th, additional specimens of this mineral not only from the "Collateral" but also from the "Chromate" veins near the former, on one of which are seen very well defined, but very small tabular crystals, the study of which will probably show them to be the species indicated. They resemble some of the forms figured by Websky from La Plata, province of Cordoba.* We must await the arrival of more specimens before the study of this interesting mineral can be completed.

Volborthite (?)—A single well characterized specimen provisionally referred to this species came among the products of the Collateral Mines. It exists in small botryoidal masses adhering to the polished faces of deep red quartz crystals. The streak is bright yellow. In thin scales the mineral is transparent and of a clear olive-green. The luster is vitreous and dull. No crystals were detected. Alone in the matrass it fuses readily, adhering to the glass. It gives off no water and dissolves in dilute hydrochloric acid to a greenish solution from which alcohol throws down the lead in tufts of plumbic chloride. On charcoal it fuses to a black shining bead which alone gives off lead fumes and copper appears on crushing the bead in the agate mortar. With soda it gives a globule of lead enclosing one of copper. Zinc oxide stains the coal when the assay is gently heated. It may be that it will turn out to be a new species.

An anhydrous cryptocrystalline mineral containing vanadium occurs among the "Collateral" ores. It varies in color from light yellow-brown to black-brown; gives the reactions for

* Monatsbericht der Akad. zu Berlin, July, 1880, 672.

Domeyko's *chileite*, but it is not a clay-like mineral. It yields readily a globule of lead containing a nucleus of copper. No arsenic was found. It occurs also in the "Chromate" vein.

Gold in coarse crystalline grains occurs in the quartz of the "Collateral" vein.

The Phœnix Mine, one mile east of the mine last named, furnishes specimens similar to those just described. The vanadinite is light yellow and deep orange-yellow to reddish, in large, well-formed crystals, which react for chlorine but not for arsenic. The gangue is quartz which carries gold but no calcite in the samples which have reached me.

Among the specimens sent to me by Mr. Farley on the 25th of June, which I have just examined, I find crystals of vanadinite in the gangue of both the "Collateral" and the "Chromate" veins, quite unlike those before described and very closely resembling in habit, color and form, the brilliant red crystals from the Hamburg mine in the Yuma Silver District.

The Montezuma lead mine, eleven miles east of Vulture and southwest of Collateral, abounds in vanadinite which occurs in drusy crusts of a rich deep yellow and brown color on masses of cerussite. Observed with a lens these crusts are seen to be well defined hexagonal prisms. It appears to be an abundant source for the supply of vanadium.

At "the Frenchman's Mine," a gold-bearing vein, of about 18 inches thickness, consists of deeply iron-stained quartz, showing amorphous yellow-green masses of a mineral very rich in vanadic acid, and reacting for lead, copper and chlorine. It is also hydrous. It may perhaps be *mottramite*. There is a buff colored amorphous substance with it also rich in vanadinite. Calcite occurs in the gangue.

There are other localities in this district in which vanadium is found, but the foregoing will suffice.

Mimetite occurs in considerable masses north of the Domingo mine on Castle Creek in the extreme northwest of the Vulture District, as here described. I have seen only a single mass of about 80 grams found by Mr. Farley. It was without gangue or associated species, and quite amorphous.

From "Bethesda Mine," in Los Cerillos, New Mexico, I collected in April, 1880, specimens showing greenish crusts of vanadinite in botryoidal forms and sometimes nearly black. It is there associated with wulfenite and cerussite. This vein is the southerly extension of the "Mina del Tiro," worked by Mexicans of old.

It is interesting to note the wide area over which this species is now known to exist, compared to the single locality at Zimpan, in Mexico; where Del Rio in 1803 first identified it. No doubt it will be found in equal or yet greater abundance at

other localities as the work of exploration goes on. Many years since, in a paper on the Mineralogy of the Wahsatch and other Utah ranges of mountains,* I called attention to the occurrence of the molybdate of lead (wulfenite), as replacing the phosphate (pyromorphite) among the salts of lead, the latter being rarely if ever found there. Subsequently the wulfenite of Tecoma and of Eureka, in Nevada, confirmed this generalization, and I have since had very frequent occasion to notice the wide distribution of wulfenite in New Mexico and Arizona. We may now add vanadic acid as having the same wide distribution.

Wulfenite crystals of rare beauty are found in the "Red Cloud" Mine, already mentioned as furnishing the vanadinite. The specimens sent me are from a depth of about 300 feet. They show very solid tabular crystals of large size, brilliant luster, and rich orange-yellow to orange-red color. The color at once suggests the presence of vanadic acid, like the well-known specimens from Wheatley Mines as detected by Smith. But I have not found a trace of vanadic acid in these Red Cloud or other Arizona wulfenites From the "Melissa Mine" in Silver District adjacent to the Red Cloud, wulfenite is found in octagonal prismatic forms, the basal plane being almost wanting, in some specimens, giving them the appearance of simple octahedrons. This interesting form I believe has not been before observed in any American locality. The color of the species at this locality is pure orange-red; the gangue is brown, almost black, calcite. The "Rover" is another mine of the same district which furnishes wulfenite nearly identical in form with the Red Cloud specimens, but of a little lighter orange-red color.

Crocoite-group.—Three if not four of the species of this group occur among the ores of the Vulture region, and especially in the "Collateral" and "Chromate" veins. These two veins together with the "Blue Jay" and the "Phœnix mine," form a group of singular mineralogical interest, furnishing, among more common minerals, the species, crocoite, phœnicochroite, vauquelinite, joassite (?), vanadinite, volborthite (?), Descloizite (?), Chileite (?), wulfenite. *Vauquelinite* occurs quite abundantly associated with galenite and crocoite in a gold quartz gangue or vein stone. The genesis of the chromate is very manifest. The nucleus of unaltered galenite is surrounded with a bright pea-green and apple-green areola of vauquelinite, sometimes semi-transparent, and uncrystalline. This green mass is succeeded by crystalline and transparent crocoite of orange-red and cinnabar-red color giving the familiar scarlet and chrome yellow streak. The crocoite as yet has not been found well crystallized. Besides the associated species already named, occur cerusite, gold

* This Journal, III, iii, 195.

and magnetite. The magnetic sand collected in washing the gold out of the crushed vein stone was examined for chromite without success.

It is an interesting question, whence came the chromic acid? Perhaps an analysis of the galenite may detect chromium in that species. Smith has described a meteoric chromium sulphide, Daubréelite,* and there is no chemical reason why this species may not coëxist with galenite. In the paragenesis of the chromates in this district the change has evidently proceeded from without inward, and the occurrence of specimens in which the whole of the galenite is transformed is not unfrequent, as also the change of the crocoite to the lemon-yellow phœnicochroite.

Small orange-yellow crystals occur in the vauquelinite of the Vulture region, which may be the *joassite;* but more study is required before they can be proved to be this mineral.

In conclusion I will add that before the study of these interesting localities can be complete a personal visit must be made by a mineralogist to the mines, and sufficient material obtained on the spot to allow of chemical analyses.

2. Thenardite from Rio Verde, Arizona Territory.

Some months since I received a lump of a saline mineral marked "Salt," reported by my informant, Mr. Treadwell, of Phœnix, to occur in abundance on the River Verde, in Maricopa County. It proved, on examination, to be anhydrous sodium sulphate or *thenardite*, a species which has hitherto been found in very limited quantity. In an analysis in the Sheffield laboratory under the supervision of Prof. O. D. Allen, by Mr. Geo. M. Dunham, its constitution was found to be as follows:

	I.	II.
Chlorine	0·095	0·097
SO_3	56·410	56·310
CaO	0·120	0·130
MgO	0·021	0·023
Na_2O	[42·964]	[43·070]
Insoluble	0·390	0·370
	100·000	100·000

I. $Na_2O : SO_3 = 6·91 : 7·02$; II. $Na_2O : SO_3 = 6·93 : 7·01$.

The mineral is therefore nearly pure Na_2SO_4. The insoluble matter out, the impurities are only 0·24 per cent of the mass.

The specific gravity of a fragment quite free from visible impurity, taken in petroleum, I find to be 2·681. This mineral occurs in large masses, some of which, in the rough, are distinctly crystals with imperfect faces, showing eminent cleavage in the direction of the basal plane of the prism and a hackly cleavage in the opposite direction. The "insoluble"

* This Journal, III, xii, 109.

matter (=0·38 per cent) in the mineral—chiefly clay, gives it a prevailing shade of yellowish gray. Its hardness is below that of calcite; luster vitreous; fracture conchoidal to hackly.

Occurring in an almost rainless country, it has suffered little change, small portions only at surface being altered to a dry white powder of exanthalose, Na_2SO_4, $2H_2O$.

As this is, so far as known, the only locality of this species where it exists in great abundance, I have taken steps to secure all the information available respecting it. Mr. Thos. F. Hopkins, of Vulture, Arizona, has forwarded to me the following statement which I present in his own words, in a letter dated—

"VULTURE MINE, A. T., June 18, 1881.

"Mr. Boyd, a resident of the Verde Valley during the past five years, is quite familiar with the large thenardite deposit, and furnishes the following details.

"The 'Salt Mine,' as it is popularly called, is situated about two and one-half miles southwest of Fort Verde (the present post), on the west side of the Verde River. Squaw Peak is distant eight miles from Fort Verde, and this mine lies between these two points.

"It occurs on a 'bench' about fifty or sixty feet above the Verde River, and itself forms one of a series of benches gradually sloping toward the river. The deposit crops out boldly in the face of the bank, and seems to extend along a distance of from eight hundred to one thousand feet. It occurs in a white chalky-looking formation, and the surface opening is probably about ten feet wide. From this opening immense masses have been carried away during more than five years past, every rancher of the district taking off huge wagon loads for the use of his stock, etc. The deposit is solid, and is removed by blasting. It is not under water at any time, for both its sloping situation and its elevation above the river forbid such a condition.

"No systematic openings of the deposit have ever been made, and hence its extent is not known. It is simply 'gouged out' according to the whim and convenience of each new comer; but it seems practically inexhaustible."

I am informed by several persons who have seen this thenardite used for the salting of cattle—among them my friend Mr. James Douglass, Jr., who was recently in that portion of Arizona—that the animals resort to it very freely, licking it as they are wont to do common salt, and with only good results.

Thenardite has been found in Nevada and elsewhere in the arid regions of the West Coast, but not before in sufficient quantity to be of commercial importance.

G. vom Rath (Zeitsch. f. Kryst.,) has lately named Lake Balschasch, in Central Asia, as a locality the flat shores of which furnish *thenardite* in very considerable quantity.

New Haven, July 8, 1881.

ART. XXXIII.—*Liquefaction and Cold produced by the mutual reaction of Solid Substances;* by Miss EVELYN M. WALTON.

THE mixing of two dry, finely-powdered salts, one or both containing water of crystallization, is often attended by liquefaction with decrease of temperature which in many instances is very marked; and sometimes there is also a decided change in color.

A transparent, homogeneous liquid is sometimes, though rarely, obtained, but generally the liquid holds in suspension an insoluble compound or an undissolved salt either in the hydrous or anhydrous state; and sometimes the consistency is that of a stiff paste.

History.—It has long been known that freezing-mixtures may be made by mixing some salt with ice or snow, and in 1875–6 Guthrie* determined the lowest attainable temperature of quite a large number of such mixtures.

He found that the lowest temperature obtained with any given salt was the same whatever its initial temperature; also that within certain wide limits this was independent of the proportions used.

The earliest allusion I find made to freezing-mixtures, formed by the use of salts only, is in the ninth volume† of this Journal, where Ordway, in a paper on *Nitrates*, mentions experiments in which the mixture of ammonium bicarbonate with hydrated iron nitrate and with hydrated aluminum nitrate was followed by a reduction of temperature from 58° to −5° F., and from 51° to −10° respectively. Subsequently‡ he mixed nitrate of iron with Glauber's salt and obtained a reduction of 32° Fahr.

Berthelot, in his recent work on Thermo-Chemistry, devotes a brief space to the subject, and the Comptes Rendus, vol. xc, pp. 1163, 1282, contains a communication from Ditte calling attention to this wonderful phenomenon. He considers the use of concentrated acids with hydrated salts, also mixtures composed solely of salts. An example is given of ammonium nitrate and hydrated sodium sulphate mixed together in a mortar, the loss of heat being about 20° C.

Liquefaction of Salts.—As far as we know, when any salt soluble in water is mixed with ice liquefaction is sure to follow, and the minimum temperature is below 0° C. But, when salts only are taken, the case is different.

In some instances liquefaction is very evident, in others there is none at all, and in still others it is doubtful; while the

* Phil. Mag., xxix, 314. † II, ix, pp. 30, 31, 33. ‡ II, xxvii, p. 15.

loss of heat is sometimes great, sometimes very slight, according to the amount of liquefaction. Whether moistening will take place or not must be decided in nearly every case by actual trial, and in the preliminary experiments made with reference to this point I have mixed the substances in a wedgwood mortar.

From a large number of trials the following conclusions have been drawn:

1. As a rule it is necessary to liquefaction that one of the solid substances used should be hydrated.

2. It is not necessary that each solid should be a salt. Moistening sometimes follows the mixing of a salt with an acid, a salt with a base, or a base with an acid.

Ex.—Calcium chloride ($CaCl_2 . 6H_2O$) with tartaric acid ($C_4H_6O_6$).
Sodium sulphate ($Na_2SO_4 . 10H_2O$) with potas. hydrate (KOH).
Potassium hydrate (KOH) with tartaric acid ($C_4H_6O_6$).

3. As when in the case of liquids, metathesis will take place if a compound insoluble in the menstruum can be formed, so with solids, if such a compound can result, metathesis is probable with liquefaction.

4. If, by mixing two salts, an insoluble compound is produced, a mixture of two others like the new ones formed will not, as a general thing, be attended by liquefaction.

5. When no insoluble compound is formed four bodies are probably contained in the product, metathesis being partial; for it is sometimes observed that liquefaction seems equally marked whether the two original salts are mixed or the two bodies formed by their interchange.

6. The rule among liquids in regard to weak and strong acids and bases seems to prevail with solids also, their action tending to promote or impede liquefaction.

7. When, by the admixture of two salts, oxydation or reduction can take place, there is again probability of liquefaction.

Ex.—$SnCl_2 . 2H_2O$	with	$HgCl_2$	liquefied.
"	"	$Fe_2Cl_6 . 12H_2O$	"
"	"	$CuCl_2 . 2H_2O$	"
"	"	$PbCl_2$	no liquefaction.

In the last no change by reduction is possible.

A new substance.—A notable exception to the rule mentioned above, that one salt at least should be hydrated, is that of $AgNO_3$ mixed with $HgCl_2$. When these are rubbed together there is decided moistening, which would seem to prove that there is such a body as anhydrous nitrate of mercury liquid at ordinary temperatures. On adding water a large residue of silver chloride is observed.

Evidences of chemical change.—When salts capable of metathesis are mixed, in addition to liquefaction, change of color, formation of an insoluble compound, and escape of a gas are proofs of chemical reaction.

An important difference sometimes noticed between mixtures of salts in the solid and the liquid form is the escape, in the former case, of some gas, as $C_2H_4O_2$, CO_2, HCl or NH_3. The gas is dissolved by a liquid solution and eludes observation.

Classification.—Cases of liquefaction may be divided into two classes; the first including those in which there is mutual exchange of base or acid; the second, those in which there is no interchange.

The mixture of lead nitrate with sodium carbonate is an example of the first class. There is metathesis, and we obtain lead carbonate, sodium nitrate, and ten equivalents of free water.

$$Pb(NO_3)_2 + Na_2CO_3 . 10H_2O$$
$$= PbCO_3 + 2NaNO_3 + 10H_2O.$$

Hydrated product.—When iron nitrate is mixed with calcium chloride thirty-six equivalents of water in some form are obtained.

$$Fe_2(NO_3)_6 . 18H_2O + 3[CaCl_2 . 6H_2O]$$
$$= Fe_2Cl_6 + 3Ca(NO_3)_2 + 36H_2O.$$

Having mixed equivalent weights, the product was dried on the smooth surface of a plate of plaster of paris which absorbed the moisture, and an analysis showed the two new salts obtained to be hydrated.

Therefore,

$$Fe_2(NO_3)_6 . 18H_2O + 3[CaCl_2 . 6H_2O]$$
$$= Fe_2Cl_6 . 12H_2O^* + 3[Ca(NO_3)_2 . 4H_2O] + 12H_2O.$$

This experiment was repeated with a mixture of $Fe_2(NO_3)_6 . 18H_2O$ with NaCl; also of $Ca(NO_3)_2 . 4H_2O$ with $MgSO_4 . 7H_2O$, and the first product was found to contain iron chloride, the second nitrate of magnesium, both in the hydrated form.

At the more or less low temperature due to liquefaction, there is naturally a tendency for salts to crystallize out from the saturated solution.

The crystalline character is sometimes perceptible to the senses, for the product often contains grains much coarser than did the finely-powdered salts first taken.

Effect of temperature.—Experiments show that sometimes liquefaction takes place readily at a temperature somewhat elevated, but not at all at a low temperature. A mortar and

* It was found that $Fe_2Cl_6 . 12H_2O$, and not $Fe_2Cl_6 . 6H_2O$, was formed.

pestle which had been warmed by hot water were occasionally used, care being taken that the heat should not be great enough to cause either of the original salts to melt in their water of crystallization.

When two salts capable of metathesis are mixed, chemical action apparently begins immediately at every point of contact. But there is a limit to the fineness of division which may be effected by mechanical means, and the substance consists of minute grains coated on the outside with the new product, while remaining unchanged at the interior.

When liquefaction ensues, the interchange is continued either because by removing the particles of the product, new surfaces are presented, or because the liquid, penetrating the granules, separates them into their molecules.

If the salts taken furnish little or no water in excess of that required to combine with the new ones formed, the process of interchange apparently soon ceases, unless sufficient heat is supplied to prevent the constituents of the product from assuming the solid form.

Difficultly soluble salts.—When salts difficultly soluble are used, moistening follows but slowly if at all. The molecules of such substances are not easily separated with a limited supply of water, especially at a reduced temperature.

Liquefaction without chemical reaction.—The second class referred to above includes mixtures of salts of the same base, or having the same acid; and although it seems to be the exception rather than the rule that there should be liquefaction in such cases, yet this sometimes occurs.

Ex.—	$Fe_2Cl_6 . 6H_2O$	with	$Fe_2(NO_3)_6 . 18H_2O$	liquefied.
	$Fe_2Cl_6 . 12H_2O$	"	"	"
	$FeCl_2 . 4H_2O$*	"	"	"
	$FeSO_4 . 7H_2O$	"	"	"
	"	"	$Fe_2Cl_6 . 6H_2O$	"
	$Na_2C_2H_2O_2 . 6H_2O$	"	$PbC_2H_2O_2 . 3H_2O$	"
	"	"	$K_2C_2H_2O_2$	"
	"	"	$ZnC_2H_2O_2 . 3H_2O$	"
	$Na_2SO_4 . 10H_2O$	"	$ZnSO_4 . 7H_2O$	"
	"	"	$Li_2SO_4 . H_2O$	"
	$CaCl_2 . 6H_2O$	"	$Fe_2Cl_6 . 12H_2O$	"
	"	"	$CuCl_2 . 2H_2O$	"

Some interesting experiments with caustic soda (NaOH) showed that when it was used with any hydrated sodium salt the combined water was liberated, evidently to satisfy the affinity of NaOH for water.

KOH was also used with various hydrated salts, and in

* Of course a ferric and a ferrous base are not strictly the same, but ferrous nitrate is too unstable a body with which to work except in the coldest weather.

every instance liquefaction ensued. Apparently the hydrated salt was attacked for the sake of its water, and the first reaction seems to be appropriation of water by KOH, which is doubtless followed by metathesis in most cases.

Liquefaction in the examples given above, however, cannot be explained in this way. Neither is there metathesis, and evidently double salts are not formed.

Having mixed equivalent weights of $ZnSO_4 . 7H_2O$ and $Na_2SO_4 . 10H_2O$, the composition of the resulting solid part was found not to be that of a double sulphate, there being an excess of Na_2SO_4.

Equivalent weights of $CaCl_2 . 6H_2O$ and $Ca(NO_3)_2 . 4H_2O$ were mixed, also of $Fe_2Cl_6 . 12H_2O$ and $Fe_2(NO_3)_6 . 18H_2O$, with a view to analysis, but in each case the thin liquid disappeared entirely into the plaster plate used for absorption, leaving only a stain visible.

Theory.—These examples must be similar in nature to mixtures of salts with ice, which result in liquefaction, and solution of the salts.

That the cold produced when ice and a salt are mixed is due to rapid liquefaction of the ice is plain enough, but I have seen no attempt made to explain the cause of the liquefaction, until Ordway last year announced his theory of the "diffusion of solids," in an address* before the American Association for the Advancement of Science.

We know that the molecules of a body are in a state of constant oscillation, and that if a salt solution be placed in contact with pure water, diffusion takes place until the molecules of salt are equally distributed throughout the mass.

So, too, when the solid is placed in water, solution follows, or, in other words, diffusion. Now, when a salt and water, both in the solid form are in contact, there is probably the same tendency to interpenetration. But a mixture of water and salt molecules cannot remain in the solid form except at a low temperature, and the rigidity of the solid state is overcome, because oscillations of the water and the salt molecules coöperate to produce a greater motion.

Graham found that although sodium chloride is not at all deliquescent, yet the saturated solution has a great affinity for water. Therefore when the smallest quantity of the salt is once in solution the first step is taken and the melting of the ice continues rapidly. If this is the true explanation of the action of sodium chloride on ice the problem is solved.

When salts capable of metathesis are used this physical phenomenon is complicated by chemical reaction. Liquefaction probably results when $CaCl_2 . 6H_2O$ and $Ca(NO_3)_2 . 4H_2O$ are

* Proceedings Amer. Assoc. Adv. Science, vol. xxix, p. 293.

mixed, and in similar cases because the crystallizing point of these two bodies together is lower than for each alone; just as the freezing point of salt water is lower than that of fresh water, and as the fusing point of an alloy is sometimes below that of either of its constituents.

Calorimeter.—For further experiments in which the reduction of temperature might be measured with some degree of accuracy, it was desirable to secure a closed space in which radiation and convection should be reduced to a minimum, and the heat of the surroundings should be constant. A calorimeter was therefore constructed somewhat like that used by Berthelot in some of his investigations.

It consists of a covered circular tank of fourteen-ounce tinned copper, of about twelve gallons capacity, placed in a much larger wooden case, the space between the walls of the tank and case being filled with loose cotton.

The upper surface of the tank has four wells, each to receive a cylindrical vessel of polished german silver resting on cork supports and having an air space around and under it. Each of these vessels, containing a glass beaker of smaller diameter (also on cork rests), is furnished with a closely-fitting cork cover, perforated to admit a thermometer and a slender wooden stirrer consisting of an upright rod with cross arms at bottom, like a pug-mill.

The thermometers have each a long stem with the scale on the upper part so that readings even to $-40°$ C. can be taken without raising the bulb from the mixture.

The tank is kept filled with water,* and this is frequently agitated by a stirrer moved with a crank. The stirrer revolves horizontally in the bottom of the tank, and having two blades like a propeller, it agitates the water thoroughly from bottom to top, the moistened part being always immersed. Over the whole is a closely-fitting wooden cover also perforated for the thermometers and stirrers.

The salts to be mixed, after finely pulverizing, were placed in separate beakers within the calorimeter, and left for a time to acquire a uniform temperature. The contents of one beaker were then added to those of the other, the cover replaced as quickly as possible, and the whole mixed vigorously by twirling the stirrer. Liquefaction generally took place in five to ten minutes and observations of time and temperature were then taken, slight agitation being still continued. There being four beakers two experiments can be carried on at the same time, and as the cover is not in a single piece one portion can be removed without uncovering the other pair of beakers.

* Water of any desired temperature may be used.

Equivalent weights were taken, seventy grams being used at first, but this was afterward increased to one hundred grams.

From the following observations it will be seen that the amount of radiation and convection is so small that it may be disregarded:

Mixture of $Mn(NO_3)_2 . 6H_2O$ with $Na_2CO_3 . 10H_2O$. Temperature of water of calorimeter, 18° C.

Time.	Temperature.
0 min.	19° C.
4	— 9°
5	—10°
6	—10·5°
10	—10·5°
11	—10°
21	— 7°
25	— 5°

Six minutes were required to reach the lowest point and during the next five minutes there was a gain of but 0·5°. Stirring was stopped at the end of eleven minutes.

Lowest attainable temperature.—In addition to Guthrie's discoveries already mentioned, he found that when two salts were used with ice the minimum temperature was unlike that of either alone, each exercising an influence over the other.

Most of my experiments with the calorimeter were made for the purpose of discovering whether or not the lowest attainable temperature of a given salt when mixed with ice is the same if that salt is produced in a freezing-mixture of two salts; also if it is independent of the initial temperature and the proportions used.

The hydrated sulphate and carbonate of sodium were each mixed with various nitrates, whereby nitrate of sodium was produced and a sulphate or carbonate, usually an insoluble compound, which I thought could not influence the result.

The lowest attainable temperature of sodium nitrate with ice is −17° C.

The following results were obtained with metals whose carbonates are without doubt anhydrous, insoluble compounds:

			Initial temp.	Lowest temp.	Loss.
$Pb(NO_3)_2$	with $Na_2CO_3 . 10H_2O$		19° C.	−17°	36°
"	"	"	0°	−17°	17°
$Ba(NO_3)_2$	"	"	21·3°	−13·7°*	35°
"	"	"	−1°	−17°	16°
$Al_2(NO_3)_6 . 18H_2O$	"	"	14°	−18°	32°
"	"	"	−4°	−18°	14°
$Cu(NO_3)_2 . 6H_2O$	"	"	16·5°	−18°	34·5°
"	"	"	−2°	−15°†	13°

* An insufficient quantity was taken. † Liquefaction proceeded very slowly.

With the nitrates of zinc, manganese, iron and chromium the results were not so free from modifying influences as I had anticipated, basic carbonates being formed not wholly insoluble at low temperatures.

The interesting fact was thus revealed that ferric carbonate or basic carbonate exists in the liquid form at a low temperature, say $-20°$ C. The color is a deep red, and as the mixture gradually warms CO_2 is rapidly given off, causing the contents of the beaker, which was not at first more than half filled, to overflow and insoluble Fe_2O_3 to be deposited.

			Initial temp.	Lowest temp.	Loss.
$Mn(NO_3)_2 \cdot 6H_2O$	with	$Na_2CO_3 \cdot 10H_2O$	18°	−14°	32°
"	"	"	− 2°	−26°	24°
$Zn(NO_3)_2 \cdot 6H_2O$	"	"	20°	−16·7°	36·7°
"	"	"	− 1°	−21·5°	20·5°
$Cr^2(NO_3)_6 \cdot 18H_2O$	"	"	− 3°	−22°	19°
$Fe_2(NO_3)_6 \cdot 18H_2O$	"	"	13·5°	−17°	30·5°
"	"	"	10·5°	−17°	27·5°
"	"	"	− 3°	−24°	21°

It will be seen from these and the following results that the minimum temperature is not independent of the initial temperature; it was also found that the lowest point varies with the proportions taken:

			Initial temperature. 1st.	2nd.	Lowest temp.
$Pb(NO_3)_2$	with	$Na_2CO_3 \cdot 10H_2O$	45°	22°*	−12·5°
$Fe_2(NO_3)_6 \cdot 18H_2O$	"	"	39°	32°	− 4°
$Al_2(NO_3)_6 \cdot 18H_2O$	"	"	37°	32°	− 2°

With the nitrates of magnesium and calcium the tendency to metathesis is so slight that the liquefaction is not rapid enough to produce any great degree of cold, and with an initial temperature of —2° there is no liquefaction whatever.

The time allotted for the completion of my graduation thesis, of which this paper gives the substance, rendered it necessary to suspend for the present the continuation of these experiments. This work was undertaken at the suggestion of Professor Ordway, to whom the subject has been one of interest for some years, but whom the pressure of other duties has prevented from pursuing an investigation. He has, however, given considerable thought to the matter, one of the results of which is his theory of the "diffusion of solids." His predictions that there may be liquefaction without chemical reaction, and that the product obtained from the mixture of salts is sometimes hydrated, were both confirmed by the results of my work. He devised the calorimeter which was used, and I am indebted to him also for valuable suggestions and advice.

Mass. Inst. Technology, June 3, 1881.

* The temperature of $Na_2CO_3 \cdot 10H_2O$ could not be raised so high as that of the other salts, without melting.

ART. XXXIV.—*On the Spectrum of Arsenic;* by OLIVER W. HUNTINGTON. With Plate IV. (Contribution from the Physical Laboratory of Harvard College.)*

IT has been noticed, in the case of the spectrum of nitrogen gas, that the spectrum obtained from an electric discharge of low intensity through a rarefied atmosphere differs from that obtained when the intensity of the discharge has been increased by a Leyden jar. In the case of the low tension discharge, the bands of the spectrum appear fluted on the more refrangible side; but upon the introduction of a Leyden jar into the circuit the fluted appearance at once vanishes, and the spectrum breaks up into isolated bands. This difference has been ascribed to a difference of condensation of the molecule. Now as arsenic is allied to nitrogen, it was thought the same difference might appear in the spectrum of arsenic, and we proposed to make this a subject of investigation, For this purpose, we first prepared two tubes,—one an ordinary Geisler tube, such as is used for showing the spectrum with rarefied gas; the other as shown in fig. 1 of accompanying plate, for the spark spectrum with Leyden jar. A small amount of pure metallic arsenic was introduced into each tube, and they were then repeatedly exhausted, each time replacing with hydrogen. After the final exhaustion, the tubes were heated, in order to fill them with the vapor of arsenic. But, upon passing the spark through them, we could obtain no definite or satisfactory result. The arsenic spectrum was feeble, the hydrogen brilliant, and the fluted indefinite bands which accompany the hydrogen spectrum wholly obscured the phenomenon.

Judging from the statements in Roscoe's spectrum analysis that these fluted portions of the hydrogen spectrum were accidental and due to impurities, we attempted to get rid of them in order to bring out the arsenic spectrum. We, therefore, prepared several tubes with pure hydrogen. We arranged tubes with two outlets, in order to pass a continuous current through the whole apparatus, including the Sprengel pump which was connected with one of the openings. The hydrogen was prepared from pure zinc and sulphuric acid, and most carefully dried. We would allow the gas to slowly pass through the apparatus for twenty-four hours, then exhaust, and after exhaustion heat the tube as hot as practicable under the circumstances, then pass dry hydrogen and repeat the process several times. Notwithstanding these precautions, we found, after a great many trials with different tubes, that the fluted

* From the Proceedings of the American Academy of Arts and Sciences, Boston, 1881, p. 35.

and more or less diffused spectrum always accompanied the four principal hydrogen lines. It being then impossible to eliminate the diffused spectrum, we next tried alloying the platinum electrodes with arsenic, and experimented with these in a rarefied atmosphere of hydrogen, both with continuous discharge of Ruhmkorff coil, and with interrupted discharge with Leyden jar. We now obtained very definite arsenic bands, apparently the same in both cases; but the effect was momentary, and gave no opportunity for measurement. The spectrum while it lasted was very striking; but, as soon as the arsenic on the extreme point of the electrode passed off, the characteristic spectrum disappeared.

We were by this experience led to contrive the following apparatus, by which we obtained the desired result, and the same may be useful in experiments on the spectra of similar volatile substances. A longitudinal section of the tube, one-half of the original size, is shown in fig. 2 of plate. The portions AA′ and A″ are of rather coarse thermometer tubing. BB′ is a tube left open at B, and drawn to a capillary point at B′. The substance to be examined, after being reduced to powder, is introduced through the opening at B until the tube is about half full. Then one end of a platinum wire is buried in the substance, and the other end is fused into the tube at B, thus closing the opening. After the hydrogen has been allowed to flow through the tube a sufficient length of time, the opening at A is closed by a nipper-tap, and the tube is exhausted at B″. Now upon connecting B with the negative electrode, and C with the positive electrode, of a small induction coil, we have the vapor of the substance in the tube BB′ carried in the current through the tube A′ where the spectrum may be observed.

One advantage of this particular form of tube is, that, in order to compare the spectrum of the substance with that of hydrogen, we have only to reverse the current, making C the negative pole, and then all the lines except those of hydrogen at once disappear.

The arsenic spectrum thus obtained is very brilliant, and consists of numerous well-marked sharply defined bands. The bands are most numerous and brilliant in the green, and these give the prevailing tone to the spectrum. But there is one very striking yellow band, and there are also several bands in the blue and violet. Then in the red there is an interesting double band, the two members of which are the same distance apart as the two D lines. In addition, there may be also a more or less diffused spectrum, which in some parts cannot be distinguished from the similar diffused spectrum of hydrogen, and it is worthy of remark in this connection, as indicating the

purity of the material used, and also that the diffused spectrum above referred to cannot come from the material of the tube, that no trace of the sodium line was seen. No account was taken of the diffused spectrum, as it appeared only when the battery was unusually strong.

In speaking of the diffused spectrum of arsenic, we do not mean the same kind of diffused spectrum as mentioned above in connection with nitrogen. The diffused arsenic spectrum appears to be composed of innumerable faint lines, wholly independent of the other more brilliant characteristic arsenic bands; and we use the term "diffused" only for convenience, to express that the lines are very faint and too numerous to measure. And we wish to call particular attention to the fact already intimated, that the spectrum of arsenic as it appears with the silent discharge bears no resemblance to the fluted spectrum of nitrogen, but consists of sharply defined isolated bands, the more prominent of which, at least, are not altered when the intensity of the discharge is increased by a Leyden jar.

The arsenic employed had been carefully purified by sublimation, and preserved under distilled water. We used for measuring the .wave-lengths of the spectrum lines the spectroscope described by Professor J. P. Cooke.* In this instrument, the train of prisms can be adjusted accurately to the angle of minimum deviation, which was observed in each case. We used five flint prisms of 45° angle each, and to reduce the angular measurements to wave-lengths, we employed the method described by W. M. Watt in his "Index of Spectra."

In the first place, we measured with care the angles of minimum deviation of the most prominent Fraunhofer lines, and verified and somewhat multiplied the data by measuring also the angles for characteristic lines of the hydrogen, lithium, sodium, thalium and strontium spectra. These we combined with the wave-lengths of the same lines given by Ångström, by ordinates and abscissas in the usual way, and the curve drawn through the points so determined was so regular and of so small curvature, that it was easy to interpolate with minutes of arc to five *tenth-metres* of wave-length, as usually expressed.

The instrument is capable of reading to five seconds of arc, and with the full bank of ten prisms it would give the wave-lengths to tenth-meters with perfect accuracy. With the comparatively feeble light of the arsenic spectrum, as we first observed it, we did not think it advisable to use the full power of the instrument. We therefore used five prisms, as stated, and read to one minute of arc. We always began each series of observations by setting the cross-wire of the micrometer on

* This Journal, xl, November, 1865.

the sodium line, after the telescope had been adjusted to the angle of minimum deviation of this line as first observed. There was seldom any observed difference in this angle. But when, by change of temperature, or otherwise, an alteration of two or three minutes had taken place, we found, on readjusting the cross-wire, that the relative position of the spectrum lines was, to the limit of accuracy of our measurement, wholly unchanged.

We give below the table of wave-lengths of the principal lines of the arsenic spectrum.

6023	tenth-meters.	5230	tenth-meters.
6013	"	5195	"
5853	"	5163	"
5833	"	**5103**	"
5813	"	5013	"
5743	"	4941	"
5653	"	**4623**	
5563	"	**4593**	'
5498	"	4493	"
[5340]	"	4463	"
5323	"	4313	"
5245	"		

The wave-lengths printed in heavy type denote the bands which are most brilliant and give character to the spectrum. The other lines are less constant and less distinct, and in some instances may be due to accidental causes.

We were surprised to find among the bright lines, that the one which in the table is enclosed in brackets corresponds to the green thalium band, and upon examining the spectrum it appeared evident that thalium must be present in the arsenic in large quantities, as the thalium band was fully as bright as any of the arsenic bands.

The diagram, fig. 3 of Plate IV, gives some idea of the general appearance of the arsenic spectrum.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Spontaneous Oxidation of Mercury and other Metals.*—Berthelot has submitted to experimental verification the question so long discussed without final settlement, whether mercury dissolves the oxygen of the air and oxidizes, even at ordinary temperatures. Perfectly pure mercury was placed in a rectangular dish of porcelain exposing a surface of $500\overline{cm}^2$ about, and covered loosely with paper. After 48 hours at a temperature of 10°, the metal yielded a slight pellicle to a tube of glass passed over it. This was removed from day to day and showed on analysis the presence of mercurous oxide. The slow oxidation of

pure mercury in contact with air can no longer be doubted. The same is true of iron, zinc, cadmium, lead, copper and tin. Now thermic data explain this phenomenon. For each equivalent of oxygen fixed by the metal, iron (rust) evolves 31·9 calories; tin 34·9; cadmium 33·2; zinc 41·8; lead 26·7; copper 21·0, and mercury 21·1. This oxidation in the air, however, is not appreciable in the case of metals whose heat of oxidation is small. Silver, for example, evolves only 3·5 calories per equivalent of oxygen absorbed. The fact that a reaction begins spontaneously only in the case where a notable evolution of heat takes place, is a result not unfrequently observed; seeming as if there was a certain resistance to be overcome, a certain preliminary work to be accomplished in order to determine the reaction. But this action becomes more prompt and more easy when an auxiliary agent is made to intervene capable of combining, with evolution of heat, with the substance at first formed; so that the total energy in action becomes greater. This is the action called in early times pre-disposing affinity. If, for example, mercury be placed in a flask and hydrogen chloride gas be mixed with the air in contact with it, the walls of the flask will be covered after a time, with mercurous chloride. Now hydrogen chloride alone does not act on mercury at all, under these circumstances; the oxygen of the air intervenes, the reaction Hg_2+HCl gas, $+O=Hg_2Cl+HO$ liquid evolving 53·4 calories.* So silver, which is not acted on by oxygen alone, is easily converted into chloride in presence of hydrogen chloride gas in addition, the reaction $Ag+HCl$ gas$+O=AgCl+HO$ liquid evolving 41·7 calories. So silver in contact with air is attacked by a solution of sodium chloride, copper by hydrochloric and acetic acids, lead by acetic acid, etc. In the case of mercury and hydrogen sulphide, in presence of air, the hydrogen is oxidized, the sulphur finely divided is precipitated and acts upon the metal. The same mechanism takes place with silver.—*Bull. Soc. Ch.*, II, xxxv, 487, May, 1881. G. F. B.

2. *On Hesperidin, a Glucoside of the Aurantiaceæ.*—TIEMANN and WILL have examined at length a glucoside found by Pfeffer in the fruit of *Citrus vulgaris* and *Citrus medica*, and called hesperidin. It appears to be universally diffused through the family of the Aurantiaceæ and is most readily prepared from the dried, unripe officinal orange (*Fructus aurantii immaturi*). The coarsely pulverized fruit was extracted with water so long as lead acetate gave a precipitate in the extract. The residue was then treated with a mixture of equal volumes of alcohol and water, containing 1 or 2 per cent of sodium hydrate, until the solution was no longer colored. From this last solution, mineral acids precipitate crude hesperidin. This is boiled with 90 per cent alcohol to remove coloring matters and the residue is dissolved in very dilute potash solution, and precipitated by a slow current of carbon dioxide. It is well washed and dried. As thus

* The symbols in these equations represent equivalents, not atoms.

obtained, hesperidin is a white, odorless and tasteless mass, consisting of fine microscopic needles, insoluble in ether and nearly so in water. Alcohol takes up only small quantities, though by distillation off of the solvent it may be obtained in somewhat larger needles. It fuses at 251° and decomposes. On analysis it gave the formula $C_{22}H_{26}O_{12}$. It possesses weak acid properties, is soluble in alkalis and reprecipitated by acids. On heating with water and sodium amalgam for a few minutes, filtering the orange solution and adding an acid, a precipitate falls which dissolves in alcohol with a magnificent red violet color, with a blue violet fluorescence. By the action of dilute sulphuric acid hesperidin splits into dextrose and hesperitin, $C_{16}H_{14}O_6$. This, by the action of alkali, splits into phloroglucin and hesperetinic acid $C_{10}H_{10}O_4$. Fused with potassium hydrate this acid yields protocatechnic acid $C_7H_6O_4$. Methyl-hesperetinic acid, oxidized by permanganate, gives veratric acid (dimethylprotocatechnic acid). Acethesperinic acid when thus oxidized yields isovanillic acid. Hence hesperetinic acid is identical with isoferulaic acid. From these reactions the author gives the following as the rational formula of hesperetin: $C_6H_3\left\{\begin{array}{l} CH{=}CH \perp CO-O \\ OH(3) \\ OCH_3(4) \end{array}\begin{array}{r} \\ (3)HO \\ (5)HO \end{array}\right\}C_6H_3$. The quantity of hesperidin which is contained in the dried fruit, about 10 per cent, suggests the importance of this glucoside to the growth of the plant.—*Ber. Berl. Chem. Ges.*, xiv, 946, Apr., 1881. G. F. B.

3. *On a new series of Volatile Organic Bases.*—MEYER and TREADWELL, by the reduction of nitrosoketones by sodium-amalgam or by tin and hydrochloric acid, have produced a series of well characterized bases of the formula $C_nH_{2n-1}N_2$, which distil without decomposition and form with water crystallized compounds. The name ketines is proposed for these bases, and one member of the series, dimethylketine, has already been described by Gutknecht, who obtained the platinum salt pure.—*Ber. Berl. Chem. Ges.*, xiv, 1150, May, 1881. G. F. B.

4. *Photometry of the Fraunhofer lines.*—VIERORDT employs the peculiar slit of his spectrophotometer to measure the relative intensity of the Fraunhofer lines, using the simple fact that the strength and sharpness of these lines varies with the width of the slit of the collimator. His paper consists merely of a preliminary note, and measurements are promised; he believes that the variation in light-intensity of the dark lines will prove the most characteristic feature of the spectra of heavenly bodies.—*Annalen der Physik und Chemie*, No. 6, 1881, p. 338. J. T.

5. *Intensity of Sound.*—OVERBECK has endeavored to obtain quantitive measurements in acoustics by the use of the microphone. It is evident that if we possessed a sufficiently delicate electro-dynamometor an electrical measure of the intensity of sound waves could be obtained. In place of such an instrument Overbeck uses a galvanometer which is affected by the varying resistance of the microphone when the latter responds to sounds

of different intensity. It is found that the microphone, used in this way, is far more sensitive than the ear to changes of tone—that it can be used with great effect to study resonance—the reflexion of sound in different rooms, and the influence of the change of temperature upon the propagation of sound waves. The author proposes to extend his investigations.—*Annalen der Physik und Chemie*, No. 6, 1881, p. 222. J. T.

6. *Reversal of the lines of Metallic Vapors.*—Professors LIVEING and DEWAR have succeeded in reversing ten of the brightest lines of iron, in the blue and violet, by passing an iron wire through one of the carbons between which the electric arc is formed. When iron is put in a lime crucible through which the voltaic arc is formed, and fragments of magnesium are dropped in from time to time, most of the strong ultra violet lines of iron are reversed. The magnesium appears to supply a highly reducing atmosphere, and to carry the iron vapor with it. It also appears to produce a continuous spectrum in certain parts, and against this the iron lines are sometimes depicted on the photographic plates sharply reversed. Potassium ferrocyanide introduced into the arc acts in a similar manner. Iron wire fed through a perforated pole reverses certain lines (wave length 2492 to 2480) and spreads out the lines into broad absorption bands. These effects are enhanced by leading into the crucible, through the perforated upper carbon, a gentle stream of hydrogen gas.—*Nature*, June 30, 1881, p. 206. J. T.

7. *Change of State.*—There are two types of change of state which are usually recognized: the ice water type, in which the change takes place first at the surface and gradually extends, the ice remaining solid up to the melting point, and the sealing wax type, in which softening takes place throughout the entire mass, on elevation of temperature. Mr. J. H. POYNTING defends the solid liquid type theory. He shows that it is easy to give an explanation of the phenomenon of melting and freezing by supposing, on the theory of the passage of molecules, "that if the temperature is not at the melting point the substance in the state with the greater vapor-tension will lose at the expense of the state with the less vapor-tension." The alteration of the melting point by pressure is explained by the supposition that pressure alters the vapor-tension, and therefore the rate of escape of molecules, and that this alteration is different for the two states. Mr. Poynting gives, on this supposition, a new proof of Sir W. Thomson's formula, which expresses the relation between the vapor-tension at plane surfaces of a liquid and the vapor-tension of the same liquid above its surface in capillary tubes. The remarkable result is deduced that if ice can be subjected to pressure while the surrounding water is not so subjected "the lowering of the melting point per atmosphere is about $11\frac{1}{2}$ times as great as when both are compressed." An account of certain experiments is given which appear to support this theoretical conclusion. A possible explanation of Professor Carnelly's "Hot Ice" is deduced from

considerations of the isothermals for ice water. The place of this "hot ice" would seem to be represented by the prolongations upward of the ice isothermals beyond the horizontal line to where they meet the line of no pressure. The critical point, which is roughly fixed at 14°C., would then be above the limit to the temperature of hot ice in a vacuum. "It is also pointed out that the sealing wax type of melting is probably similar to the change of ice into water below the lower, or above the upper, critical points, if these exist."—*Phil. Mag.*, July, 1881, pp. 32–48. J. T.

II. Geology and Mineralogy.

1. *Geology of the Province of Minas Geraes.*—From two important memoirs published by Prof. Henrique Gorceix in the *Annaes da Escola de Minas de Ouro Preto*, noticed in our last, we condense the following account of the geology of the central part of the province of Minas Geraes, Brazil.

The greater part of the central portion of the province of Minas Geraes is constituted by the great chain appropriately named Serro do Espinhaço. This chain is formed principally of quartzose and schistose rocks, to which are joined granitic gneiss and even true granites, mica schists, dikes and intercalated beds of diorite and finally small deposits of anomalous rocks containing tourmalines, disthene and other minerals.

The quartzose rocks are true quartzites consisting of irregular grains of hyaline quartz without cement. To the quartz in these rocks are united two other substances, a green mineral and micaceous iron which serve to characterize two principal geological horizons. The inferior division of the quartzites is characterized by the presence of a soft green unctuous mineral generally described as talc, but which unlike talc contains only an insignificant proportion (1 to 3 per cent) of magnesia with a large proportion of alumina, and the alkalies, potash and soda. The presence of small quantities of iron, manganese and chrome probably determines its green color. These quartzites are known by the name of itacolumites and are in the lower division characterized by a schistose or flaggy structure.

In the quartzites with the green substance two subdivisions are recognized at Ouro Preto. The lower one consists of flaggy beds which near Ouro Preto are inclined at an angle of 25° or 30° to the southward. The second and more important division constitutes the peak of Itacolumi, and consists of more massive beds with an easterly inclination. Both divisions are traversed by auriferous veins, in which the matrix is generally common iron pyrites or arsenical iron pyrites.

In some cases, as at Morro Velho, Pary, etc., quartz enters in relatively small proportions in the vein matter and the gold is very fine, and in small but constant quantity. When, on the contrary, the pyrites disappear and the vein is formed almost exclusively of quartz, the gold is in larger grains but very irregularly disseminated in the vein rock.

The second division of the quartzites is characterized by the substitution of the green matter by micaceous iron and often by the disappearance of the quartz; these pass to beds of iron ore known by the name of *itabirite.* The beds of itabirite attain in places the thickness of more than 200 metres and by the abundance and purity of the mineral and the facility of extraction constitute the richest iron ore deposits of the world. The iron is often accompanied by oxide of manganese which in places enters in a proportion as high as 9 per cent, or more.

In the friable itabirites gold is often found disposed in a manner which seems to be peculiar to Brazil. The gold appears disseminated in the rock in scales analogous to the scales of iron oxide, these scales being sometimes joined together so as to form large nuggets. The distribution of the gold in the rock appears to be irregular but it is probable that the rich lines have, like veins, a definite direction. The absence of sulphides which characterize the gold bearing rocks inferior to itabirites, is worthy of note. The only substance which appears to mark the presence of gold is a white lithomarge appearing in little pockets in the rock.

The schistose rocks are of very variable characters, and when fully studied, either from a geological or mineralogical point of view, will fall into several divisions. They are generally shales passing at times to true slates; soft, greasy to the touch and of various colors, green, yellow, red, black, etc. These schists have generally been described as talcose, but analysis proves them to be argillaceous, rich in alkalies and with but a trifling proportion of magnesia.* True talcose rocks consisting of soapstone or potstone are however met with in small basins in the midst of the schists. The schists may be divided into two groups with reference to their relations to the itabirites, namely, those below the itabirites characterized by brilliant mica-like scales, extreme softness, and a relatively small development of the schistose structure, and those superior to the itabirites characterized by a greater predominance of the argillaceous character and of the schistose structure.

These schists are everywhere metamorphosed, but in the north of the province in the Jequitinhonha and Arassuahy basins the alteration of the rocks is more pronounced than in the region farther south and the rock becomes crystalline, passing to mica schist and other types of crystalline rocks. These crystalline schists perhaps belong to another geological series. This change to the crystalline character is accompanied by the appearance of numerous veins of quartz accompanied by tourmalines, staurolites, spodumene, chrysoberyls, etc.

In the series of schists the gold-bearing veins are less numerous than in the other groups described, and are of inconstant richness. In places gold also appears distributed in the rock in a manner analogous to that in the itabirites but this only occurs in the parts contiguous to the latter rock. The group of schists is also

* These are evidently *hydromica* schists.- -EDS.

characterized by the presence of isolated masses of crystalline limestone or marble.

The determination of the geological age of these various rocks, and even that of the relative ages of the different groups, is rendered difficult by the absence of fossils, and by the excessive dislocation of the beds by folding and faulting, faults being particularly numerous and giving a peculiar character to the mountains of the region, which generally present a moderate slope on one side and a precipice on the other.

The rocks above described have been referred to the Tertiary and Secondary ages; but there are good reasons for considering them more ancient than the limestones of the São Francisco in which Prof. O. A. Derby found fossil corals which indicate that these are much older than the Secondary and belong to the Paleozoic age.

The more modern rocks are represented by the peculiar iron conglomerate denominated *canga* formed on the surface from the fragments of the underlying rocks and which continues to form to-day, and by deposits of lignite of Tertiary age as is proved by the fossil plants and fishes contained in them.

A fact of considerable interest, from an agricultural point of view, is the uniform presence of a notable proportion of alkalies, particularly potash, in all the schistose rocks examined, and the absence of lime in the same rocks. The first fact explains the wonderful fertility of many of the soils derived from the decomposition of the schists, and the second indicates the proper fertilizer for the more sterile soils.

Of the precious stones found in Minas, the deposits of topazes, situated near Ouro Preto, have been most studied. Topazes and the still rarer euclases are found in their primitive formation in a small basin west of Ouro Preto in which several mines have been opened. The rocks of this region consist of schists and quartzites with the green substance, the beds being inclined at angles of 30° to 50° to the eastward. The schists are the predominant rocks and belong to the two divisions already described of clay schists and greasy or unctuous schists. They contain pyrophyllite and embedded octahedral crystals of iron oxide having the form of and resulting from the alteration of pyrites.

The various topaz mines that have been opened lie along two parallel lines running W.S.W. In the Boa Vista mine which is a deep open cut, the beds explored are unctuous shales of several varieties containing the talc-like mineral already mentioned. These beds are inclined to the eastward at an angle of 40° to 50° and are covered by superficial deposits of sand and conglomerate. The gems occur in an irregular fracture or vein filled with a soapy clay or lithomarge and running about W.S.W., or perpendicular to the strike of the country rock. The vein divides into branches, some of which sometimes accompany the bedding, and is often split up into pockets in which the topazes are of greater size and more abundant. Rarely topazes are found without the lithomarge

in a brown clay rock to which the gem-bearing veins appear to be confined. The other minerals accompanying the gems are quartz in fine crystals often penetrated by the topaz crystals, specular iron and very rarely euclases of which only seven or eight were found in the extraction of several kilograms of topazes. In the other mines examined the conditions are essentially the same, the presence of crystals of rutile being noted in one of them.

The topazes are generally of the well known yellow color though it is not rare to find reddish ones; light green and colorless crystals are also found, but very rarely. The relation with the lithomarge is so intimate that layers of this substance are often found penetrating the cleavage planes of the crystals. Other crystals having the composition of topaz are brown and opaque or with a slight yellow varnish on the surface, without well defined cleavage, and pass into a bluish schist which occurs in blocks in the mass of the unctuous schists.

The diamond appears to belong to the same geological horizon as the topaz, accompanying in its distribution the quartzites or so-called itacolumites. It has not been found in the immediate vicinity of Ouro Preto but the diamond-bearing zone commences about sixty kilometers north of that city and extends almost due north for a long distance, following the divide between the waters of the São Francisco and the coast rivers. The idea that the quartzites or the itacolumites form the primitive formation of the diamond is an old one and arose from the fact that these rocks are the predominant ones in the diamond region, but neither the gem nor its attendant minerals were seen by the early explorers in their original position.

The origin of the diamond may be studied by means of the accompanying minerals, which being more abundant can more readily be traced to their place of origin. Of these, some may be regarded as accidentally associated with the diamond, but others, whose presence in the gem-bearing gravels is more constant, must be regarded as true satellites. Among these last the minerals containing titanium such as anatase, rutile, rutile pseudomorph after anatase, and titaniferous iron hold the first place. To these are to be added black tourmaline, hematite in the form of specular iron and of octahedral crystals, magnetite in grains, and, in some places, klaprothine, in others, platinum. All of these minerals, with the exception of the last, have been found in the quartz veins which are very abundant in the neighborhood of Diamantina, cutting the quartzites and schists.

The diamond also occurs in quartzite near the city of Grão Mogol, where mining was at one time carried on. A specimen of this rock containing a diamond has long existed in the national museum at Rio and two specimens have lately been obtained for the collection of the School of Mines. The rock in these specimens consists of irregular grains of quartz with flakes of mica or of the green substance, and with embedded crystals among which is the diamond.

In its lithological characters it resembles closely the upper quartzite of the Serra de Itacolumi and probably belongs to the same geological horizon.

Two theories may be proposed to account for the presence of the diamond in this quartzite. One that the diamond already existed when the rock was consolidated and thus entered into its composition like any other pebble; the other that the diamond was formed in the rock. At first sight the first theory appears the most probable one, but there are some reasons for giving more credit to the second.

A third mode of occurrence was noted by Messrs. Heusser and Claraz at São João da Chapada, near Diamantina, where the diamond is associated with a white clay analogous to lithomarge which occurs with veins of quartz containing specular iron, that traverse the quartzites.

It will be seen, therefore, that the diamond and topaz are found in the same rocks and geological position and with the same mineral associates.

The other colored minerals or gems of Minas, viz: the beryl, chrysoberyl, spodumene, andalusite, garnet and red and green tourmaline, occur in an older series of crystalline schists which is formed to the east of the diamond-bearing zone in the basins of the Jequitinhonha and Arassuahy. The rocks of this region consist of gneiss and mica-schists which in places become graphitic. The gems occur principally in loose gravel but have been traced to their original deposits in quartz veins traversing the crystalline schists.

It is to be noted that of these minerals the tourmaline is also associated with the diamond and topaz-bearing rocks, but in this case it is always the black variety, not the red, green or white varieties of the crystalline schists.

In concluding this brief abstract of the very interesting investigations of Prof. Gorceix, by far the most complete and serious studies that have ever been made of the geology of Minas and the mode of occurrence of the precious stones which have rendered the province famous, we would say that for the most part his conclusions are in complete accord with those of our countryman, Prof. O. A. Derby, who visited the diamond region last year and who has now in press a memoir giving the results of his studies. In the few minor points in which the two geologists are not in accord further investigations are necessary, and we are pleased to be able to state that the eminent geologist of Ouro Preto has just undertaken a trip to the northern part of the province in which it is to be hoped he will have the satisfaction of completing his studies and of setting at rest the long disputed questions in regard to that most interesting subject, the mode of origin and occurrence of the diamond.—*Editorial in Rio News*, *Rio de Janeiro*, May 24th.

2. *Progress of the Volcanic Eruption on Hawaii.**

The great eruption of Mauna Loa has been flowing for about eight months. The mighty mountain has poured forth from its upper vents, near Mokuaweoweo, the summit crater, a river of lava, about fifty miles long and varying from half a mile to four miles in width, which is now distant a few miles from Hilo, threatening to destroy the town, to fill up the harbor, and probably, as on a former occasion of eruption, invade the Pacific ocean and add many thousand acres to the area of the Archipelago. Whilst seeking for compensation in the view of a possible great misfortune, it may be interesting to note, that whilst King Kalakaua is making the tour of the world, in order to bring more people under his beneficent sway, the goddess Pele may be adding a new appanage to His Majesty's dominions.

The latest reports from the eruption inform us that the great lava flow that had reached within two miles of Hilo, had then broadened its stream to a width of about four miles, and banked it up in places to a height of over one hundred feet, and there halted, like a beleaguering force, before making a final assault, and storming the doomed city. Already it had sent off a skirmishing stream, the narrow flow running down the gulch of Kukuau; and should the great lava embankment burst forth along its front, the destruction of Hilo would be swift and overwhelming, with not a vestige upon the corrugated and wavy surface of black glass and clinker to show that over the spot, the aspirations and spires of a christian community once pointed to heaven.

We learn from recent visitors many interesting particulars in regard to the present state of the great active crater, Kilauea, which is distant about thirty miles from Hilo. Tourists to the volcano, for many years past, all remember certain active pools of lava, the North and South Lakes, which ordinarily bubbled and tossed a fiery flood at a depth of about 120 feet below the floor of the great crater; now these lakes have all been filled up, and there have arisen peaks and cones of hard lava, that rise over one hundred feet above the south bank of the great crater which is about one thousand feet high. But there has burst forth a new opening in the great crater floor, not far distant from the old lakes, and a new lake, almost round in form, about six hundred feet across and some seventy feet in depth in ordinary stages, below the surrounding brink. Here the great Hawaiian volcano presents the most varied fantastic play of liquid lava. The following are some of the phases of the play of a fire lake, as recently observed in the crater of Kilauea. Sometimes it almost seems to sleep, and the disappointed visitor looks down into a black valley and observes a smoking pit, giving no more evidence of combustion than a tar kiln. It presents a dark silver grey hue with a satiny shine. This is a crust of quiescent lava; and the observer who has expected to have his sense of wonder

* The earlier features of the eruption were announced in the last volume of this Journal, on page 79, in a letter from Rev. T. Coan, of Hilo.

strained to speechlessness, says: "Is this all?" But soon the broad disk of the lake heaves and trembles. Now the moving floor cracks and a serrated fissure, like the suture of a skull, runs from side to side; and quick darting streaks, sudden cracks of the crust, shoot across in all directions. These serrated streaks are, at first, rosy lines on the gray surface; then they are wider, like crimson ribbons, broadening to the view. Another crimson fount springs up along the now fretting and roaring rim of the lake. And another, and another of now wildly upleaping fountains of fire toss high their ruddy crests, and throw off gouts and clots of red spray that fall and harden near the observer's feet. By this time the spirit of our inferno is aroused. The whole fierce red lake is all boil and leap and roar. It is more than the roar of loud sea surfs beating bold bluffs. The surging tide of the molten earth, sounds a deeper, bellowing bass than any note of the sounding sea. Finally the heaved up crust broken into fragments, is churned up and dissolved in the boiling flood. The roaring gulf is now indeed a vortex of indescribable glories and terrors.

And then the wild lake settles down to calm again or to a milder display by and by; or perhaps simply upheaves, and overflows its bounds and spreads abroad in the great crater. But at all times it is wonderful, and is ready to satisfy the curious observer that here in mid Pacific, in our Hawaiian islands, is the grandest, most varied and most momentous volcanic action to be seen on the surface of the globe.—*Letter to the Commercial Advertiser*, *Honolulu*, July 30.

From a Letter of Rev. Titus Coan, dated Hilo, June 28.—For a few days past our volcanic fires have been more vivid and glaring than ever.

The northern wing of the line is less than six miles from us, and the southeastern is less than five miles distant, while the center of the line appears the most sanguinary. From the southeast wing the lavas have fallen into a rough water channel, twenty to fifty feet wide, which comes down from the main bed of the flow almost direct to Hilo, rrossing Volcano street, half a mile from Mills' store and entering into the Waialama stream, which cuts the beach about midway. In this way the lava at white heat is fast approaching the shore. It is now only two and a half miles from Volcano street, and it is very liquid, running much like water. It has, some part of the time, run at the rate of half a mile a day.

I have been to the lava flow to-day (June 28th) and returned. We found two streams of liquid lava coming down in rocky channels which are sometimes filled with roaring waters, but nearly dry at this time. These two gulches are too small to hold the flowing lava, and the fiery flood overruns the banks, and spreads out on either side. The united width of these streams may vary from fifty to two hundred feet. In going down the steeper parts of these rocky beds the roar is like that of the

heavy surf on the coast, and often like thunder.—*Hawaiian Gazette*, July 6.

Letter of D. H. Hitchcock, dated Hilo, June 30th, 1881.—About Wednesday of last week, the old mountain was observed to be more than usually active, the whole summit crevasse pouring forth immense volumes of smoke. By Friday noon the three southern arms had all joined into one, and rushing into a deep but narrow gulch forced its way down the gulch in a rapid flow. By Saturday noon it had run a mile. On Monday morning it was reported to have reached the flats, back of Halai Hills. Monday afternoon we rode up to it before dark and found that the stream was entirely confined to the gulch and intensely active. It was than about half a mile from the flats spoken of.

The flow was on an average 75 feet wide and from 10 to 30 feet in depth, as it filled the gulch up level with its banks. The sight was grand. The whole frontage was one mass of liquid lava carrying on its surface huge cakes of partly cooled lava. Soon after we reached it the flow reached a deep hole, some 10 or 15 feet in depth with perpendicular sides. The sight as it poured over that fall in two cascades was magnificent. The flow was then moving at the rate of about 75 feet an hour. About midnight we noticed a diminution in the activity of the gulch flow and soon saw a bright red glare above the tree tops, and were presently startled by the burning *gas bursts* and the crackling and falling of the trees somewhere above us. The whole sky above was lined with the light of burning trees and shrubs. About 2 A. M. we made the attempt to reach the scene of the great activity and succeeded by going up the south side of the gulch some quarter of a mile. And what a scene lay before us as we ascended a slight elevation. The on-coming overflow had swept over the banks of the narrow gulch and was flowing like water into a dense grove of neneleau and guava trees. There they stood in a sea of liquid lava over a space of more than an acre, while the fires were running up their trunks and burning the branches and leaves overhead. The flow was so rapid that the trees were not cut down, for more than 200 feet from the front of the flow. In one place we saw a huge dome of half melted lava rise up, 15 or 20 feet high, and twice that in diameter and apparently remain stationary, while the fiery flood went on.—*Hawaiian Gazette*, July 6.

Letter from Rev. Mr. Coan to Professor Chester S. Lyman, dated Hilo, July 21st, 1881.—By mail of to-day I send you the Hawaiian Gazette of the 6th inst. In it you will see the state of the lava flow of that date. Since then the southeast wing has made fearful progress. I was at the lower end of the igneous stream on the 18th inst. It was then about two miles from the upper part of our town, making slow progress toward us. One of our guests returned to us early this morning reporting that the action of last night was very great, and that the movement in the outer channel was, at one time, 60 feet in 19 minutes. He

thinks it is now only a mile from our town, and that it can be reached in 15 minutes. It now seems nearly sure that this advance will reach us. Still we have hope.

It is now $8\frac{1}{2}$ months since the outburst began near the summit of the mountain. During this period it has sent out a vast stream some 30 miles toward Mauna Kea; another of nearly equal dimensions toward Kilauea. Between these streams others of very liquid *paihoehoe* have divided and subdivided on the sides of the mountain, on the plains below, and in the great forest between the mountain and the sea. Some parts of the fiery line are still operating in the woods about five miles distant, but the southeastern wing has come through in force, and from this wing the stream which now threatens us has advanced four miles from the main body. Should its speed increase it will soon enter our town in the channel which cuts the beach about in its center and enter the harbor. But as the body of the fiery fusion is too large to be confined to the water channel, it will probably spread on both sides and thus consume many buildings.

It is amusing to see the children and even older people gathered at the lower end of the flow and along its margin, all eager to collect specimens from the viscid streams, moulding with poles the plastic mass, as the potter the clay, into various forms of cups, vases, birds, fishes, etc. These are readily sold at various prices to strangers.

3. *Glacial drift on Mt. Ktaadn, Maine.*—From a paper by C. E. Hamlin, published as No. 5 of vol. vii of the Bulletin of the Museum of Comparative Zoology (vol. i of Geological Series), entitled Observations upon the Physical Geography and Geology of Mt. Ktaadn and the adjacent district, we cite the following.—

Material interesting from its relation to the transportation of drift, whatever may have been the agent that moved it from the north, is not wanting upon Ktaadn. The two slides furnish the chief amount of such material. * * * *

On the East Slide much less drift is found than on the other. Outside of the slides, I have never found drift upon the flanks of the mountain; but it re-appears higher up, in very small amount on the Table Land, but principally upon the northern summits, sparsely strewn among the broken granite that covers them. Neither on slides nor summits is the drift ever found in large bowlders, but always as fragments of moderate size. On the Southwest Slide a *few* masses were seen as heavy as a hundred pounds each, but in general—always, upon the East Slide—the pieces run from a few ounces up to twenty pounds in weight. They were chiefly fragments of slates and sandstones, identical with the strata of the country north and west, mingled with pieces of metamorphic and trappean rocks, such as occur in place for a few miles beyond the Ripogenus Carry.

The fragments of stratified rocks on the Southwest Slide very generally include fossil shells, mainly Brachiopods, and always

impressions or interior casts. Owing to the small size of the enclosing masses—due to the fissile structure of the rocks—the fossils ordinarily are much decayed, but occasional specimens are obtained in fine condition. Among the scanty drift upon the upper third of the Southwest Slide, I have never seen a fossil-bearing stone. And upon those parts of the summits where drift was found, only once was a fossil met with,—a solitary Brachiopod impression on a ten-pound piece of sandstone, picked up on the slope northward from West Peak to the Saddle, about 600 feet below the top of the peak, or at an elevation of about 4,615 feet above the sea. This is by far the highest point at which fossiliferous rocks have yet been found upon Ktaadn.*

All the facts in the case serve to indicate that the non-granitic material found upon the mountain is a portion of the so-called "northern drift," with the fact of whose distribution—not the manner—we are here concerned. But we may and must suppose that in the distribution the sides and summits of Ktaadn, as far up at least as 4,600 feet, received deposits of drift more or less in quantity.

4. *Doleryte* (*trap*) *of the Triassic-Jurassic area of Eastern North America.*—Dr. G. W. HAWES, using Thoulet's method of separating associated minerals, through their difference in specific gravity, by means of a mixture of potassium iodide and mercury iodide in solution, has investigated the composition of a specimen of the doleryte (diabase as he names it) from Jersey City. When the mixture reached the specific gravity 3, the magnetite and augite of the finely pulverized rock, and some mixed grains, had sunk to the bottom, and only feldspar, as the microscope showed, remained at the top; and when diminished to a specific gravity of 2·69 (without any considerable portion further settling) the feldspar portion "separated into two parts with such facility as to plainly show that two minerals were present." In chemical analyses of these parts by Dr. A. B. Howe, the two yielded:

	SiO_2	Al_2O_3	Fe_2O_3	MgO	CaO	Na_2O	K_2O	H_2O	
1. Over 2·69....	52·84	28·62	1·52	0·46	11·81	2·38	0·86	1·06	= 99·55
2. Under 2·69 ..	60·54	24·11	1·14	0·27	9·15	4·11	1·06	0·59	=100·97

After citing these analyses the author remarks: "It is therefore plain that the feldspathic element in this rock is not any single feldspar. One of the feldspars is very plainly labradorite, and the other has the ratio of andesite. The two feldspars were distinguishable under the microscope, and the optical properties of

* Dr. De Laski's statement of the height (4,385 feet) at which he found fossils, "well up toward the 'Horseback' ridge" (this Journal, III, iii, p. 27), and which is quoted by Professor Dana in his Manual of Geology (editions 2d and 3d), is founded upon a wrong estimate of the altitude of the mountain. He adopted the one current for some years before Professor Fernald's remeasurement of the elevation, which he made to be 5,215 feet. Now the elevation of the "Horseback" ridge, at a point directly up from the head of the East Slide—Dr. De Laski's route—is 4,109 feet. It was *below* this point, that De Laski found his "upper fossils."

the grains offered no peculiarities to conflict with the above determination."

This method of analysis, as Dr. Hawes is aware, has a source of error in the fact that the grains of a fine-grained crystalline rock will not altogether, and perhaps not generally, be wholly free from admixture, owing to the adhesion and interpenetration of the associated minerals, and feldspars especially are likely to be thus blended; hence, while the existence of at least two feldspars is thus plainly proved, the analyses of the two parts can give only approximate results, and so they are regarded by the author.

There is another source of uncertainty as regards the feldspars of such a rock in the similarity of specific gravity of some of the species. The range for the prominent kinds, excluding some extreme numbers, are as follows:

Orthoclase	2·50—2·59	Andesite	2·65—2·72
Albite	2·59—2·63	Labradorite	2·66—2·72
Oligoclase	2·59—2·66	Anorthite	2·70—2·77

Anorthite from all its localities (with a rare exception), about half of the varieties of Labradorite, and one-third of those of andesite, have the specific gravity 2·69 or above; the rest, below 2·69. The doubts that are thus introduced, chemical analysis can in part remove.

Dr. Hawes continues as follows: "The analyses of the anorthite and augite that I picked from West Rock may be added, and our knowledge of this diabase may be said to be quite complete as regards the composition of the fresh rock. I will place together the analyses of the rock and its other components. Professor Genth's analyses, to which I have referred, is more complete than any that I have made, since he determined the traces of lithia, copper and sulphur. But his analysis was made on more hydrous material; therefore I will use my old analysis of West Rock, New Haven,* because the analyzed material was very fresh, bright and clear, and also illustrates the commonest variety of the rock."

The following analyses are then cited from the article by him just referred to:†

	SiO_2	Al_2O_3	Fe_2O_3	FeO	MnO	MgO	CaO
West Rock, New Haven	51·78	12·79	3.59	8·25	0·44	7·63	10·70
Augite, West Rock	50·71	3·55	—	15·30	0·81	13.63	13·35
Anorthite, West Rock	45·95	34·70	0·64	—	1·80	*tr.*	15·82

	Na_2O	K_2O	TiO_2	P_2O_5	Ign.	
West Rock, New Haven	2·14	0·39	1·41	0·14	0·63	= 99·89
Augite, West Rock	[1·48]		—	—	1·17	= 100·00
Anorthite, West Rock	0·45		—	—	0·96	= 100·32

* This Journal, ix, 183, 1875.

† In citing the analysis of anorthite from his former paper, several changes are made: 15·82 is placed opposite MnO instead of CaO, evidently by a slip of the pen, and this is corrected above; but, further, 1·80 is put opposite MgO, when it is the amount of K_2O in the original paper, and 0·45 is put opposite K_2O and Na_2O together, when it is the amount of K_2O in the original paper. No reason for the latter changes is given, and it remains uncertain as to which is in error.

Dr. Hawes next gives the result of a *calculation* by him of the mineral constitution of the West Rock trap from the above elements, which is as follows:

"Anorthite 15·52, albite 22·16, potash feldspar 2·32, augite 54·47, titanic iron 2·68, magnetite 1·76, apatite 0·32, total 99·23."*

Dr. Hawes thus makes out that the feldspar of the Jersey City trap consists of *labradorite* and *feldspathic material having the ratio of andesite*; while that of West Rock consists chiefly of *anorthite* and *albite*.

This extraordinary result for the West Rock trap and its so wide divergence from that for the Jersey City trap make it important to consider carefully the details in the calculation. It is the more marvellous since Mr. Hawes's analyses of the Jersey City and West Rock traps, in his former paper, gave them very nearly the same chemical constitution. We cite the analyses together for comparison, along with another (from the same paper) of a trap from Wintergreen Lake, which adjoins West Rock.

	SiO_2	Al_2O_3	Fe_2O_3	FeO	MnO	MgO
1. Jersey City	53·13	13·74	1·08	9·10	0·43	8·58
2. West Rock	51·78	14·20	3·59	8·25	0·44	7·63
3. Wintergreen Lake	52·42	14·54	1·25	9·84	0·51	7·33

	CaO	Na_2O	K_2O	P_2O_5	Ign.	
1. Jersey City	9·47	2·30	1·03	—	0·90	= 99·76
2. West Rock	10·70	2·14	0·39	0·14	0·63	= 99·89
3. Wintergreen Lake	10·59	2·23	0·49	—	0·55	= 99·75

In Mr. Hawes's citation of the West Rock analysis (see above) he deducts 1·41 from the alumina, reducing 14·20 to 12·79, on the ground of the recent finding of this amount of titanic acid in it by Dr. A. B. Howe; and if right in this, some similar deduction would probably have to be made for the rock of the other localities.

New analyses throughout would have afforded a surer basis for a calculation. But even with these, a different treatment of the facts would have been required for right conclusions.

Mr. Hawes says, in the paragraph cited above, after giving his results from the Jersey City trap, that our knowledge of this rock may be said to be quite complete after adding his analyses of the anorthite and augite which he "picked from West Rock." But anorthite found in a trap at West Rock, New Haven, and not in the Jersey City rock (places eighty miles apart), has no bearing on the composition of the latter, except by way of suggestion.

Further: the "anorthite in West Rock," of which he gives the analysis, was not from the West Rock dike, and has no where been detected in the West Rock trap, or in any other trap of the various New Haven trap ridges (or as yet elsewhere in the Connecticut valley) except in a single dike that intersects the West Rock ridge transversely and thence continues along the south side of "Wintergreen Lake," and which is therefore of later origin. The anorthite is in isolated crystals about three inches apart on

* The numbers for the anorthite and albite should be transposed.

an average, thus making the rock very sparsely porphyritic; and Mr. Hawes in his former paper (in which the locality and rock characteristics are rightly given) remarks that it crystallized out from the mass of the rock because of its different composition; as he has since rightly observed, it was first to crystallize because less fusible than the rest of the feldspar portion.*

The *mass of the rock*, containing none of the anorthite crystals, was analyzed by him separately and his results are those of No. 3 in the last table; they show a very near identity with the West Rock trap.

It appears, hence, that Dr. Hawes's recognition of anorthite as a prominent ingredient of the West Rock trap was not warranted by any observed facts; that his announcement of albite as a constituent has as yet nothing to sustain it; and that the Mesozoic trap of eastern North America still needs careful investigation.

J. D. D.

5. *New Devonian Plants.*—Dr. DAWSON read before the Geological Society of London, June 23d, 1880, a paper describing several new North American Devonian plants, as follows: A small Tree-fern, *Asteropteris Noveboracensis*, characterized by an axial cylinder composed of radiating vertical plates of scalariform tissue, imbedded in parenchyma, and surrounded by an outer cylinder penetrated with leaf-bundles with dumb-bell-shaped vascular bundles, from the Upper Devonian of New York; a species of *Equisetites* (*E. Wrightianus*), showing a hairy or bristly surface, and sheaths of about twelve, short, acuminate leaves; a specimen of wood, new in its characters, from the Devonian of New York, named *Celluloxylon primævum*, and having some analogies with *Prototaxites* and with *Aphyllum paradoxum* of Unger; also several new ferns from the well-known Middle Devonian plant-beds of St. John, New Brunswick, confirmatory of the previous conclusion as to the age of the beds, and showing the harmony of their flora with that of the Devonian of New York, and also the fact that the flora of the Middle and Upper Devonian was eminently distinguished by the number and variety of its species of ferns, both herbaceous and arborescent.

6. *On Fossil Plants from the Lignite Tertiary Formation, at Roches Percées, Souris River, Manitoba;* by Dr. J. W. DAWSON. (Canadian Naturalist, Jan., 1881).—Dr. Dawson states in his paper that the Lignite Tertiary Group of Manitoba and elsewhere in the Western Plains rests immediately on the Upper Cretaceous, and holds extensive deposits of valuable lignite, associated with shale and sandstone containing numerous remains of plants. This flora resembles very closely in its aspect that of the Miocene Tertiary of Europe, but there is reason to suspect that the whole belongs to a period of transition between the Cretaceous and Tertiary ages. The species of plants were collected by Mr.

* New Hampshire Geol. Reports, vol. iii, p. 92. He says, speaking of a similar case in New Hampshire: The reason for the occurrence of the anorthite in large isolated crystals is "that the anorthite is much less fusible; hence in rocks cooled from igneous fusion, the anorthite would crystallize first, and would have an opportunity to form larger crystals in the still plastic mass."

Selwyn, and includes the following: Leaves of a magnificent *Platanus* or Sycamore, a foot or more in length and of proportionate width, identical with *P. nobilis* of Newberry, from the Tertiary beds of Fort Clarke on the Upper Missouri; a species of *Sassafras*, a genus not hitherto found in our Lignite Tertiary, though represented in the Cretaceous and in modern times, dedicated in the paper to Mr. Selwyn; several Poplars, as *Populus arctica* Heer, *P. cuneata* Newberry, *P. acerifolia* Newberry, a Hazel, a chestnut-leaved Oak apparently new, some Coniferous trees, as *Sequoia Langsdorfii*, an ally of the giant trees of California, *Taxodium occidentale*, of Newberry, and *Taxites Olriki* of Heer. The flora indicated is, on the whole, similar to that of the Porcupine Creek group of Dr. G. M. Dawson's Report on the 49th Parallel, that of the Lignitic area of the Mackenzie River, described by Heer as Miocene, that of the Fort Union group of Newberry, and of the Carbon group of Lesquereux,—formations variously regarded as Eocene or Lower Miocene, and very widely distributed over the western plains. These plants will be fully described in a forthcoming report of the Geological Survey, where their affinities and geological relations will be discussed.

7. *North American Mesozoic and Cænozoic Geology and Palæontology, or an Abridged History of our knowledge of the Triassic, Jurassic, Cretaceous and Tertiary Formations of this Continent;* by S. A. MILLER. 338 pp. 8vo. Cincinnati, 1881. (Reprinted in volume form from the Journal of the Cincinnati Society of Natural History.)—This volume is the result of much labor. It contains a mention of a large part of the papers, memoirs or works published in the country on the geological formations mentioned in the title, with often citations of paragraphs giving the views contained, and will be of much use to geologists. The work is most complete paleontologically, as this is the particular direction in which the author has labored. The volume is not properly a history, but rather like a scrap-book in the collection of its material. The arrangement under the grand divisions is chronological; but there is much mixing up of dates and subjects under the Cenozoic, where the drift, Eocene, Miocene, etc., come in variously; and references, as well as dates, are often wanting throughout the work, or are insufficiently given. By improving it in these respects and making it complete in its list of papers, the author would increase greatly the value of the volume. On one topic—that of the drift—the work departs very widely from a history, and the references are much more defective than elsewhere. He says that "he has undertaken to overthrow the Glacial hypothesis." As his knowledge of the subject extends, he will probably reject many of his explanations, and come out, like nearly all others who have studied the subject, a good Glacialist, though his objections to some of the views which he makes part of the Glacier theory are likely to stand.

8. *Species of Pterygotus from the Water-lime group near Buffalo.*—Mr. J. POHLMAN has described in the Bulletin of the

Buffalo Society of Natural History, vol. iv, No. 1, 1881, the maxilliped of *Pterygotus Buffaloensis*, from the Water-lime group near Buffalo, with illustrations, the length 6¼ inches and breadth 1⅓; and also, from the same beds, the new species, *Ceratiocaris grandis*, the carapace measuring 9½ inches in width and 5¼ in length and having its surface finely granulose. The rock has also afforded *Eurypterus remipes*, *E. lacustris*, *E. robustus* and *E. Dekayi*. The author states that *Eusarcus scorpionis* of Grote & Pitt is probably Hall's *Eurypterus pustulosus*.

9. *On the genus Alveolites, Amplexus and Zaphrentis, from the Carboniferous System of Scotland;* by JAMES THOMSON, F.G.S. Phil. Soc. of Glasgow, 1881.—This paper gives a review of previous views as to the genus *Alveolites* and its relations to *Favosites* and *Chætetes*. It is stated to differ from the last two in having fissiparous generation, while *Favosites* differs from *Chætetes* in the presence of mural pores. The paper is well illustrated by many figures on four plates, representing details as to the corals of 4 species of *Alveolites*, 6 of *Amplexus*, and 23 of *Zaphrentis*, of which one species of *Alveolites* is first described by the author, and 9 of *Zaphrentis*.

10. *A Memoir upon Loxolophodon and Uintatherium;* by HENRY F. OSBORN, Sc.D., with *a Stratigraphical Report on the Bridger Beds in the Washakie Basin*, by JOHN B. MCMASTER, C.E. 54 pp., 4to, with 6 plates. Princeton, N. J., July, 1881.—This paper is the commencement of a series of publications, in large and handsome quarto form, under the title, "Contributions from the E. M. Museum of Geology and Archæology of the College of New Jersey." The memoir treats of the distinction and characteristics of the genera *Uintatherium* and *Loxolophodon*, and describes the new species *Loxolophodon Speirianum*, besides giving the characters of portions of the skeleton of *U. Leidianum*, and also, at greater length, of that of *U. mirabile* (which is the *Dinoceras mirabile* of Marsh). The memoir is illustrated by six excellent lithographic plates, one folded plate representing the skull of *L. Speirianum* one-third the natural size; the second, the skull of *U. Leidianum;* the third, teeth of *Loxolophodon;* the fourth, restoration of *Loxolophodon;* the fifth, a map of the Eocene basin of Wyoming Territory, and the sixth, sections through the Leclede Bad Lands.

11. *Vanadinite in Arizona.*—The following note to the editor, from W. P. Blake, was received in a letter dated San Francisco, June 14, 1881: "Will you please note in the Journal that in a letter to you I report the occurrence of vanadinite in the lead-bearing veins of Castle Dome District, Arizona, associated with wulfenite, cerussite, galena and fluor-spar."

III. BOTANY AND ZOOLOGY.

1. DECANDOLLE, *Monographiæ Phænogamarum.* Vol. III. Paris, Masson, June, 1881, pp. 1008, tab. I–VIII.—This ample volume has very promptly followed its predecessor, which contained

the *Araceæ* by Engler. This comprises four more Monocotyledonous orders, and one Dicotyledonous, namely the *Cucurbitaceæ*.

The least important of the monocotyledonous orders is that of the *Philydraceæ*, of only four Australian species (one of which is also E. Indian), divided among three genera! And one of those has had three names or even four if we count an orthographical difference. The author is the accomplished Prof. Caruel late of Pisa, now again of Florence, where he may be expected to do much good work for our science.

The three following small orders, *Alismaceæ*, *Butomaceæ*, and *Juncagineæ* are elaborated by M. Micheli of Geneva, who, in a preface treats of the literature, structure and limitation of these nearly related groups. Although for the present admitting all three to ordinal rank, the author distinctly favors Bentham's view, viz: that the second group should go with the *Alismaceæ*, and the third be kept apart. And the singular genus *Lilæa*, he would exclude as well from the *Naideæ* as from all these orders or groups, although he appends it to the *Juncagineæ*. From *Alisma* L. to *Sagittaria* there is such a succession of connecting forms that it is very questionable how many, if any, generic divisions should be maintained. But in order to sustain the Linnæan genera, Micheli adopts three intermediate ones (*Limnophyton* of Miquel, *Elisma* of Buchenau, and *Echinodorus* of Richard) and makes one new one (*Lephiocarpus* on *Sagittaria calycina* and two S. American species), besides *Damasmium*, Juss., which is well characterized by its biovulate carpels. This character is not shared by the Californian species, which therefore is remanded to *Alisma*, thus weakening the former genus. *Limnophyton* consists only of an African and Indian species, which was for Linnæus a *Sagittaria* and for Willdenow an *Alisma*, the latter preferable. *Elisma* (on *Alisma natans* L.) has a better-defined character in its introrse micropyle. *Echinodorus* is at length worked up into 17 species, two of them European, the rest American. All of N. American species extend to the tropic and most of them to Brazil. The forms of *Sagittaria* are arranged under 9 species, of which we have six, and *S. variabilis* is restored to *S. sagittæfolia*. The so-called campylotropy in most of these genera is the result of a subsequent flexion of an anatropous ovule. Of the *Butomaceæ* it only need be remarked that *Limnocharis* (including *Hydrocleis*) is referred to it. Of the *Juncagineæ* it is to be noted that the species of *Triglochin* have a very wide distribution, and that *T. triandra* of Michaux, with several synonyms, is referred to *T. striata* of Ruiz and Pavon, as indeed had been made out by Buchenau and his predecessors. *T. maritima* is said to inhabit salt or brackish swamps only. In North America it grows luxuriantly in mountain bogs of perfect freshness.

The *Commelinaceæ*, by C. B. Clarke, fill over 200 pages and are illustrated by eight lithographic plates, which are not very well executed. The 307 known species are, with apparent good judgment, ranked in three tribes, and under 26 genera, of which

the conspectus hardly exhibits the characters. The order is chiefly tropical, but it, like several others, finds its most northern limits in the Northern United States or British America. It is to be hoped and expected that our few but troublesome species of *Commelina* are here well settled. *Tinantia anomala* is the new name of Torrey's *Tradescantia anomala* of S. Texas. *Tradescantia pilosa* (*T. flexuosa*, Raf.) is made a mere variety of *T. Virginica*. *T. Floridana* of S. Watson is cited as a synonym of *T. gracilis*, H. B. K. *T. linearis*, Benth. is in Wright's collections from S. Texas. *Tradescantia leiandra* of Torrey is *Commelina leiandra* of Clarke, while Torrey's var. *brevifolia* is *Zebrina ? leiandra* of the same.

The *Cucurbitaceæ*, ably monographed by Cogniaux of Belgium, occupy two-thirds of the present volume. It is more than fifty years since this important order was elaborated for the Prodromus by Seringe, upon a tenth part of the materials now in hand. Naudin has in later years admirably elucidated a considerable number of genera, mostly upon the living plants, and sketched some of the grouping. But the full study and proper characterization of the tribes and genera, as now known to science, was the work of Sir Joseph Hooker, in the first volume of the Genera Plantarum, published in 1867, a work which receives (as it well deserves) high praise from the present monographer. Indeed, the classification of the Genera Plantarum is completely adopted; and the changes in the limitation of genera are wonderfully few and slight, considering the wealth of species and of hitherto unexamined materials which M. Cogniaux has had in hand. So completely have the extant materials been brought together, or otherwise examined, that the author is able to declare that there are only eight out of the 600 species now described which he has not seen; and also that over one-third of them (219) have been first described by him, either in the present monograph or in his recent anterior publications. Such faithful and conscientious work cannot be too much lauded and commended as an example. In the prefatory portion the various mooted questions respecting the morphology of the tendrils, inflorescence, andrœcium and fruit of the order, are referred to rather than discussed; but the whole bibliography is indicated in a foot note. Upon the andrœcium the author does express an opinion, and upon good grounds. The crucial instance is really furnished by the genus *Feuillea*, which has the full number of five stamens, wholly separate, and alternate with the petals. If their anthers were really bilocular, as Hooker in the Genera Plantarum took them to be, then it would probably be correct to say that the ordinary Cucurbitaceæ have 2½ stamens, *i. e.* two with bilocular and one with an unilocular anthers. If, on the contrary, these normal five are unilocular, we must conclude, with Payer and Baillon, that unilocularity is the type of the order, and that old notion is correct, namely: that the apparent three stamens are really five, four of them united in pairs, and one separate. Now Cogniaux is perfectly right in the

statement that the anthers of *Feuillea* are unilocular, although bilocellate. Indeed, Hooker had subsequently ascertained this in the case of *F. Moorei*, figured in the Botanical Magazine, although he does not there generalize the observation. This being the case, the older view must be preferred. And being preferred by Cogniaux, it would have been better to have adopted it practically as well as theoretically, and constructed the generic characters accordingly, instead of on the old model of "*Stamina* 3, *anthera una unilocularis, cæteræ biloculares*," more convenient though it be.

The geographical distribution of a family at once so peculiar, so wide-spread and so considerable in numbers and generic diversity (79 genera and 600 species), might raise interesting speculations. It must be an ancient family; for the numerous genera, as well as the species, are circumscribed in range, and only six or seven are common to the Old and New World, except as diffused under human agency.

We note one generic name to be changed, not because of the somewhat bizarre fancy of Mr. C. B. Clarke in naming two genera of the same order in honor of the same person, *i. e.*, *Warea* and *Edgaria*, but because the Cruciferous genus *Warea*, always of unquestioned validity, has held its place in all the books from Lindley's Vegetable Kingdom down to the new Genera Plantarum inclusive. The latter work is followed in the reference of *Megarrhiza* to *Echinocystis*, as proposed by Decaisne, although meanwhile the singular germination has been made known, confirming our opinion that the genus is a thoroughly good one. Changes in the nomenclature of our scanty North American *Cucurbitaceæ* are few. *Cucurbita perennis* is identified with the Mexican *C. fœtidissima*, H. B. K. We should say that this and the related perennial species are provided with fleshy roots, not with "*rhizomato crasso.*" *Sicydium* of Schlechtendal having been identified with *Triceratia* of A. Richard, and the genus rehabilitated, the Texan *Sicydium* of Gray and Engelmann becomes the type of a new genus. which is dedicated to that capital botanist, Maximowicz. *S. Lindheimeri* is therefore *Maximowiczia Lindheimeri*, *Trianosperma* becomes a section of an older genus *Cayaponia* of Mauso, and our species is accordingly *C. Boykinii*, Cogn. Naudin's *Echinopepon* is by Cogniaux also absorbed into *Echinocystis* (which we should restrict to the original species), and so the *Elaterium Coulteri*, *E. Wrightii*, etc., are here transferred to the first section of the latter genus. The West Indian *Sicyos laciniatus*, L., takes in *S. parviflorus*, Gray, not Willd., nor Kunth.

All the numbers of distribution of specimens which are cited in the volume are specially indexed at its close under the collectors' names, alphabetically arranged,—a great convenience. A. G.

2. *Arboretum Segrezianum: Icones Selectæ Arborum et Fruticum in Hortis Segrezianis Collectorum, etc.;* par ALPHONSE LAVALLÉE, President de la Société Nationale d'Agriculture de France, &c.—The collection of trees and shrubs at Segrez (a few

leagues from Paris), although of comparatively recent foundation by a single individual at his country place, is already an important establishment, and in hardy shrubs it is wholly unrivalled. The shrubs are not only collected, but critically studied. The valuable catalogue published a few years ago gave evidence of this. And now, in this more sumptuous work, critical, little-known, or new species are admirably figured, fully described, and their history and synonymy discussed with ability. Two volumes of 60 plates each are promised; three fascicles have already appeared (the first in 1880, the third early in the present summer), each of six plates and a sheet of letter-press, in imperial quarto form. The plates are engraved on copper from drawings by Riocreux and by one or two other artists of hardly less excellence; and all the main details of flower and fruit are given in the analysis. The pomaceous or other fleshy fruits are colored. Altogether this is a work of note and of the highest value, is evidently a labor of love and of pure scientific devotion. It is published by J. B. Baillière at Paris, etc., at the price of ten francs per part of six plates. Thus far most of the species illustrated are either North American or of North-eastern Asia, and therefore of special interest to us. The American species are as follows: *Jamesia Americana*, *Diervilla sessilifolia*, *Nuttallia cerasiformis*, *Cratægus punctata*. A. G.

3. *The British Moss-Flora;* by R. BRAITHWAITE, M.D., F.L.S. Part iv. Fam. v, *Fissidentaceæ*.—This continuation of the excellent work which we have already noticed includes pp. 64–82, and plates 10–12, and illustrates 13 British species of *Fissidens*. The plates are admirable. One of the species is *F. ventricosus* of Lesquereux, figured in the supplement to Sullivant's Icones, and here referred to the European *F. rufulus* of Schimper. A. G.

4. *Butterflies, their Structure, Changes and Life-histories with special reference to American Forms;* being an *Application of the "Doctrine of Descent" to the study of Butterflies*, with *an Appendix of Practical Instructions;* by SAMUEL H. SCUDDER. 322 pp. 8vo. New York, 1881. (Henry Holt & Co.).—This beautiful volume is popular in its style and its many excellent illustrations, and scientific thoroughout, also. Biology has no stranger or more interesting facts than those connected with the structure, development and habits of butterflies; and this is made strikingly apparent by the descriptions in Mr. Scudder's well-written and attractive work. The various topics discussed—the egg, caterpillar, chrysalis, full-developed butterfly, and the various steps in the process of transformation, their food and modes of taking it, their nest-making, and their seasonal and regional variations and other varyings unaccounted for, which seem to look toward new species, their geographical distribution and their colonization in New England—these and other subjects are illustrated almost exclusively by reference to American butterflies. The Appendix contains instruction for collecting, rearing, preserving and studying butterflies, besides a list of the species mentioned in the text and of the food plants.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Meeting of the American Association for the Advancement of Science, at Cincinnati, Ohio.*—The thirtieth meeting of the American Association opened at Cincinnati on Wednesday, the 17th of August, under the presidency of Professor George J. Brush, of New Haven, and closed on the Tuesday following. Excellent arrangements had been made by the Local Committee for the meeting and for the various conveniences of the members. One of the features thus supplied was the connecting of the rooms of the several Sections with one another by telephones, whereby the papers in progress in one Section were announced on bulletin boards in the others.

The meeting was unusually large in its attendance and every way successful. A list of the papers accepted for reading is given beyond. An able address, illustrated with lantern views, was given Wednesday evening, by Captain C. E. Dutton, on the excavation of the Grand Canyon of the Colorado, from his own explorations of the region. A resolution, laid over from the preceding meeting as required by the constitution, was adopted, dividing the association into eight sections: A. Mathematics and Astronomy; B, Physics; C, Chemistry and its applications; D, Mechanical Science; E, Geology and Geography; F, Biology; G, Histology and Microscopy; H, Anthropology; I, Economic Science and Statistics; but giving power to the Standing Committee to consolidate any two or more sections, whenever deemed advisable.

The liberality of the citizens of Cincinnati contributed largely to the pleasures of the week, and it followed the members after its close by arrangements for excursions on Wednesday and Thursday: one to the Mammoth Cave, Kentucky; another to Chattanooga and Lookout Mountain (335 miles); and a third, for the anthropological section, to the prehistoric cemetery at Madisonville where excavations have been made.

Montreal was made the next place of meeting, and the 23d of August the time. Dr. J. W. DAWSON, of Montreal, was appointed President for the meeting. The other officers elected are the following:

Permanent Secretary: F. W. PUTNAM (continued). *General Secretary:* WM. SAUNDERS, London, Ontario. *Assistant General Secretary:* Professor J. R. EASTMAN, Washington. *Treasurer:* WILLIAM S. VAUX, Philadelphia (continued).

Vice Presidents: Prof. WM. HARKNESS, Washington, Section A; Prof. T. C. MENDENHALL, Columbus, Ohio, Section B; Prof. A. C. BOLTON, Hartford, Connecticut, Section C; Prof. WM. P. TROWBRIDGE, New Haven, Connecticut, Section D; Prof. E. T. Cox, San Francisco, California, Section E; Prof. W. H. Dow, Washington, Section F; Prof. A. H. TUTTLE, Columbus, Section G; Prof. D. WILSON, Toronto, Section H; Prof. E. B. ELLIOTT, Washington, Section I.

Secretaries: Section A, Prof. H. T. EDDY, Cincinnati; B, Prof. C. S. HASTINGS, Baltimore; C, Dr. A. SPRINGER, Cincinnati; D, Dr. C. P. DUDLEY, Altoona, Pa.; E, Capt. E. C. DUTTON, Washington; F, Dr. C. S. MINOT, Boston; G, R. BROWN, Jr., Cincinnati; H, Prof. OTIS T. MASON, Washington; I, Dr. FRANKLIN B. HOUGH, Lowville, N. Y.

Prof. W. B. ROGERS was elected the first Honorary Fellow of the Association.

List of Papers accepted for Reading.

1. *Astronomy, Mathematics and Physics.*

D. P. TODD: Note on a comparison of Newcomb's tables of Uranus and Neptune, with those of the same planets by LeVerrier.

WM. HARKNESS: On the methods of determining the solar parallax, with special reference to the coming transit of Venus; On a simple method of measuring faint spectra.

M. BAKER: Alhazen's problem: its history and bibliography, together with various solutions of it.

H. A. NEWTON: Numbers of cometary orbits relative to perihelion distance.

J. R. EASTMAN: Method of determining the value of the solar parallax from meridian observations of Mars.

A. W. BROWN: The saroscope; A register of eclipses traced from 3939 B. C.

S. S. PARSONS: Electricity, magnetism, gravitation—their phenomena considered as the manifestations of one force.

J. D. WARNER: Scheme for aiding the memory of Euler's transformations of coördinates.

P. E. CHASE: Universal energy of light.

E. B. ELLIOTT: On standard time.

W. W. PAYNE: Time service, Carleton College Observatory.

J. D. WARNER: Symmetrical method of elimination in simple equations, by the use of some of the principles of determinants.

S. J. WALLACE: On an abbreviation in writing, a long series of figures, and its use in calculations; On a sign of logical connection in equations.

E. L. NICHOLS: On the electrical resistance and the coëficient of expansion of incandescent platinum.

H. T. EDDY: A preliminary investigation of the two causes of lateral deviation of spherical projectiles, based on the kinetic theory of gases; Note on the theory of flight of elongated projectiles; On the mechanical principles involved in the flight of the boomerang; On a new method of applying water power of small head to effect the direct compression of air to any required high pressure.

S. MARSDEN: Experiments to determine the comparative strength of globes and cylinders of the same diameter.

W. LECONTE STEVENS: An improved sonometer; The stereoscope, and vision by optic divergence.

T. C. MENDENHALL: On the wave-lengths of the principal lines of the solar spectrum; Note on an experimental determination of the value of π; Remarks upon and an exhibition of Japanese magic mirrors.

J. R. PADDOCK: A new self-registering mirror barometer.

J. LAWRENCE SMITH: The needle telephone, (a new instrument by Dr. Goodman, of Louisville, Ky.); An anomalous magnetic property of a specimen of iron; Nodular concretions in meteoric iron, bearing on the origin of same.

A. G. BELL: Upon a new form of electric probe: Upon the use of the induction balance as a means of determining the location of leaden bullets imbedded in the human body.

R. H. THURSTON: On the effect of prolonged stress upon the strain in timber.

J. E. HILGARD: On recent deep-sea soundings in the Gulf of Mexico and Caribbean Sea, by the U. S. Coast Survey.

F. E. NIPHER: Magnetic Survey of Missouri.

G. W. HOLLEY: Suggestions for improvement in the manufacture of glass, and new methods for the construction of large telescopic lenses.

E. L. STURTEVANT: Four years observation with a Lysimeter, at Farmington, Mass.

L. WALDO: A new theory of the formation of hail; On the errors to which self-registering clinical thermometers are liable.

H. C. HOVEY: A remakable case of retention of heat by the earth.

O. STONE: On the great outburst in Comet *b*, 1881, observed at the Cincinnati Observatory.

WM. BOYD: A musical local telegraph alphabet.

T. BASSNETT: Numerical elements of the orbits of the seven electrical vortices, to whose motions atmospheric storms are principally due.

T. STERRY HUNT: Historic notes on cosmic physiology.

H. CARMICHAEL: A new radiomotor; A new differential thermometer.

2. *Chemistry.*

H. W. WILEY: Amylose; its nature and methods of analysis; Relation of reducing power as measured by Fehling's solution to the rotatory power of glucose and grape-sugar (amylose); Mixed or new process sugar, with methods and results of analyses.

J. LAWRENCE SMITH: Determination of phosphorus in iron; Regulator of filter pumps; Iron with anomalous chemical properties; Hiddenite, a new American gem.

C. RICHARDSON: The nitrogenous constituents of grasses.

W. O. ATWATER: Chemistry of fish and invertebrates; Quantitative estimation of nitrogen; Quantitative estimation of chlorine; Sources of the nitrogen of plants.

A. B. PRESCOTT: The limited biological importance of synthetic achievements in organic chemistry; Notes in experimental chemistry.

R. B. WARDER: Evidence of atomic motion within molecules in liquids as based upon the speed of chemical action.

A. SPINGER: Pentachloramyl formate.

C. F. MABERY and RACHEL LLOYD: Dibromiodacrylic and chlorbromiodacrylic acid.

MRS. A. B. BLACKWELL: Constitution of the "Atom" of science.

MISS V. K. BOWERS: Is the law of repetition the dynamic law underlying the science of chemistry?

C. F. MABERY and H. C. WEBER: On chlortribrompropionic acid.

H. B. PARSONS: Composition and quality of American wines.

C. W. DABNEY, JR.: An iso-picraminic acid.

H. C. HOVEY: Coal dust as an element of danger in mining.

FR. A. ROEDER: An attachment for burettes, avoiding the necessity of using glass stop-cocks; On a new form of balances.

H. CARMICHAEL: A filtration evaporation balance; The liquefaction of glass in contact with water at 250°.

G. C. CALDWELL: Some new forms of apparatus for the chemical laboratory.

P. COLLIER: Development of sugars in maize and sorghums.

S. W. ROBINSON: Ringing fences.

3. *Geology and Natural History.*

E. W. CLAYPOLE: The evidence from the drift of Ohio in regard to the origin of Lake Erie; On the discovery of an Archimediform Fenestellid in the Upper Silurian rocks of Ohio.

S. W. TROBRIDGE: Remarks on the classification and distribution of Producti.

WM. H. BALLOU: Natural and industrial history of the White pine in Michigan; Niagara River, its cañon, depth, and wear.

WM. MCADAMS: Fossil teeth of mammals from the drift of Illinois; The occurrence of Cretaceous fossils near mouth of Illinois River.

H. CARMICHAEL: The temperature of North German traps at the time of their extrusion.

W. J. McGee: A contribution to Croll's theory of secular climatal changes.
H. S. Williams: The recurrence of faunas in the Devonian rocks of New York; On some fish remains from the Upper Devonian of New York.
R. Owen: The unification of geological nomenclature.
Edw. S. Morse: On changes in Mya and Lunatia since the deposition of the New England shell-heaps.
J. W. Dawson: On Ptilophyton and associated fossils from the Chemung Shales of Ithaca, N. Y.
E. Orton: The Berea Grit of Ohio.
N. H. Winchell: Typical thin sections of the rocks of the cupriferous series in Minnesota.
G. C. Swallow: Ozark Highlands.
G. Sutton: Gold-bearing drift of Indiana.
J. W. Spencer: Features of the region of Lower Great Lakes, during the Great River age: or notes on the origin of the great lakes of North America.
Wm. Bross: Cañons, with some thoughts as to their origin.
H. Allen: Revision of the anatomy of the ethmoid bone in the Mammalia.
C. F. Gissler: On *Bopyrus Manhattensis* from the gill-cavity of *Palæmonetes vulgaris* Stimpson.
B. G. Wilder: On a mesal cusp of the deciduous mandibular canine of the domestic cat, *Felis domestica.*
H. D. Schmidt: On the influence of the structure of the nerve-fibres upon the production and conduction of nerve-force.
C. S. Minot: Note on the segmentation of the vertebrate body; Note on whether man is the highest animal; Relations of the growth, size and age of animals.
A. J. Howe: Digital differentiation.
Wm. Zimmerman: Recent existence of sword-fish, shark, and dolphin in the fresh water pond near Buffalo, N. Y.
S. A. Forbes: On some relation of birds and insects.
Wm. H. Brewer: On the disposition of color-markings of domestic animals.
Mrs. L. Stone: Notice of a fern indiginous to California, but heretofore considered as an introduced hot-house species.
C. E. Ridler: Some needed reforms in the use of botanical terms.
D. P. Penhallow: Phenomena of growth in plants.
W. J. Beal: The motion of roots in germinating Indian corn.
T. Meehan: Additional facts on the fertilization of Yucca.
B. D. Halsted: The lift unit in plants.
C. E. Dutton: Cause of the arid climate of the far West.
H. C. Hovey: Recent discoveries, measurements, and temperature observations made in Mammoth Cave, Ky.
D. W. Prentiss: On the action of Pilocarpin in changing the color of the human hair.
G. C. Swallow: Natural filtration of water for domestic use in cities.
E. S. Edmunds: Evolution and its place in geology.
D. D. Thomson: Influence of forests on streams.

4. *Entomology and Microscopy.*

E. W. Claypole: Life-history of the Buckeye stem-borer.
C. V. Riley: Retarded development in Insects; New insects injurious to American agriculture; The egg-case of Hydrophilus triangularis; On the Oviposition of Prodoxus decipiens; The cocoon of Gyrinus.
W. H. Edwards: On certain habits of Heliconia charitonia; On the length of life of butterflies; On an alleged abnormal peculiarity in the history of Argynnis Myrina.
J. A. Lintner: On the life duration of the Heterocera (moths); A remarkable invasion of northern New York by a Pyralid insect—*Crambus vulgivagellus.*
A. J. Cook: How does the bee extend its tongue; The Syrian bees; Carbolic acid as a preventive of insect ravages.
B. P. Mann: Suggestions of coöperation in furthering the study of entomology.
T. Taylor: New freezing microtome; Bacteria and micrococci, and their relations to plant culture.

G. M. STERNBERG: Contribution to the study of Bacterial organisms commonly found on exposed mucous surfaces in the alimentary canal of healthy individuals.

L. CURTIS: A study of blood during a protracted fast.

WM. A. ROGERS and G. F. BALLOU: On a convenient method of expressing micrometrically the relation between English and metric units of length on the same scales.

C. S. MINOR: The best method of mounting whole chick embryos.

ROBT. BROWN, JR.: On a convenient form of slide case (with specimen).

J. D. COX: Some phenomena in the conjugation of the infusorium *Actinophrys sol.*

5. *Anthropology.*

O. T. MASON: The uncivilized mind in the presence of higher phases of civilization.

H. HALE: A lawgiver of the Stone Age.

W. C. HOLBROOK: Mound-builders' skeletons; Prehistoric hieroglyphics; Stone implements in the drift.

WM. MCADAMS: The stone images and idols of the mound-builders: Remarkable relics from mounds in Illinois; Stone implement showing glacier marks.

W. H. DALL: On the inhabitants of N. E. Siberia, commonly called Chukchis and Namollo.

J. G. HENDERSON: Houses of the ancient inhabitants of the Mississippi Valley; Was the antelope hunted by the Indians on the prairies of Illinois? *Ilex cassine*, the black drink of the Southern Indians; Agriculture and agricultural implements of the ancient inhabitants of the Mississippi Valley.

MRS. ERMINNIE A. SMITH: Comparative differences in the Iroquois group of Dialects; Animal myths of the Iroquois.

E. S. MORSE: On the ancient Japanese bronze bells; On worked shells in New England shell-heaps.

W. J. HOFFMAN: Interpretation of pictographs by the application of gesture-signs.

S. H. TROWBRIDGE: Exhibition of archæological specimens from Missouri.

C. THOMAS: On worked shells in New England shell-heaps; Comparison of Maya dates with those of the Christian era.

A. S. GATSCHET: Phonetics of the Káyowē language.

W. DE HAAS: The mound-builders—an inquiry into their assumed southern origin; Antiquity of man in America; Progress of archæological research.

S. D. PEET: Buffalo drives on the Rock River in Wisconsin: The emblematic mounds on the four lakes of Wisconsin.

F. W. LANGDON: The temporal process of the malar bone in the ancient human crania from Madisonville.

2. *Science Observer and a cipher-code for Astronomical telegraphic messages.*—The Science Observer, published by the Boston Scientific Society, under the immediate editorship of J. Ritchie, Jr., and devoted to the publication of Astronomical news, and especially whatever is of immediate importance to the working Astronomer, contains, in its last issue (Nos. 9 and 10 of vol. iii), a paper by S. C. Chandler and Mr. Ritchie on a new form of writing telegraphic messages for transmitting astronomical data. The method makes it possible to send messages containing astronomical detail, such as the elements and ephemeris of a comet, without any danger of error. A dictionary is used in making out the code-cipher—Worcester's Comprehensive Dictionary, edition of 1876. This book contains 390 pages, with over 100 words to a page; consequently, any integral number, up to 39,000, can be represented by a word; for example: 16,718, by the 18th word on page 167, which is *electrize;* 349° 12′, by the 12th word on page

349, which is *proportionableness;* April $14^d\ 10^h\ 48^m$ (=April $14^d{\cdot}45$,=134·45 day of the year (or 135·45, on leap year)), by the 45th word on page 134, which is *crush*, and so on. In a similar manner each position of an ephemeris can be represented by two words, one for the right ascension and one for the north polar distance, which is to be preferred to declination, as the distinction of plus and minus is thereby avoided. We refer to the article for the details of the plan by which it is adapted to all the requirements of general astronomical work. It has already been put into use between Boston and the Dun Echt Observatory. For one example: The elements and ephemeris of Comet (*b*) 1881, computed at Boston, were communicated to Lord Crawford, at Dun Echt, in the words —elegy pyrrhic linger armillary bass illiteracy needy calmness supervention chary stonework comprehensibleness staggard curse spondaical confest diapente. The word illiteracy was a *control word*, introduced to show whether the elements had been correctly received. It is the word in the dictionary which corresponds to one-fourth of the sum of the numbers expressing the elements,—a fourth being taken so that "the number may be always within the limit of a 400-page book." The publisher of the Science Observer announces that he will supply copies of the paper containing all the details as to the code-cipher, printed on heavy paper for observatory use, for 25 cents each, and will send a copy of the Dictionary, post paid, for $1.25.

The last number of the Science Observer contains also Elements and Ephemeris of Comet (*b*) 1881, and a report of observations on the same comet, by O. C. Wendell, assistant at Harvard College Observatory; Elements and Ephemeris of Comet (*c*) 1881, from the Harvard and other observatories, with other Astronomical intelligence. The Science observer is published at the low price of 50 cents for twelve numbers, which make a volume.

3. *A Dictionary of the Exact Sciences, Biographical and Literary;* by J. C. Poggendorff—continued and completed.—Dr. W. Feddersen, of Leipzig, is preparing a supplement to Poggendorff's well-known biographical dictionary. Many of our readers will receive during the next few days circulars asking them to answer a few questions as to their scientific life and labors. As the great utility of such a work lies in the completeness of the information it supplies, it is to be hoped that all appealed to will send full answers to the questions, allowing neither false modesty nor carelessness to cause a failure.

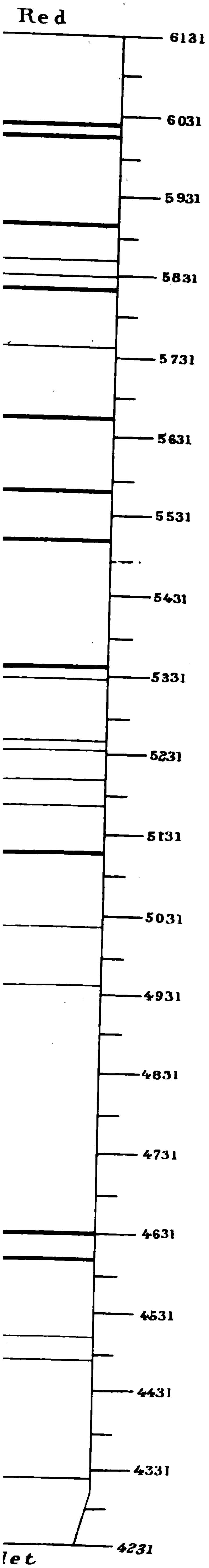
4. *Report of the Cotton Production of the State of Louisiana, with a discussion of the general Agricultural features of the State,* being an extra Census Bulletin; by Eugene W. Hilgard, Prof. Agric. Univ. of California. 100 pp., 4to. Washington, 1881.—Professor Hilgard's geological explorations of the States of Mississippi and Louisiana, and his study at the same time of their agricultural resources, have eminently fitted him for the work he is doing with reference to cotton production for the Census Reports. The report just issued reviews first, by means of tables,

the amount of production of the leading crops in Louisiana; and then gives a brief outline of the physical geography of the State; a description of the great alluvial plain of the Mississippi, and of the agricultural regions of the State, together with analyses of soils and a discussion of the same; separate agricultural descriptions of the several parishes under the heads of the agricultural regions to which they belong; and, lastly, information as to agricultural practice in the several parishes, obtained as replies to a series of questions under various headings; these replies afford data for a comparison of the different parts of the State, as regards these several points. The questions relate to depth of tillage; the draft used in breaking up; the practice of subsoiling; fall plowing or not; rotation of crops or not, and if so, the order, and the results; kinds of fertilizers, and the results; use made of cotton seed, and its price; preparation of cotton land before bedding up; planting time; planting in ridges or not; variety of seed preferred; amount of seed per acre; what implements; what after-cultivation; time of first blooms; time of picking; and so on, followed by other questions with reference to ginning, baling and shipping; diseases, insect enemies; labor and system of farming; wages, etc. The report shows that the great subject of cotton production could not be in better hands.

5. *Third Bressa Prize, Academy of Turin, open to Scientists and Inventors of all Nations.*—The value of the Bressa prize is 12,000 francs. The third prize is to be given to the person, of whatever nationality, not a member of the Academy, who, during the four years 1879–1882, shall have made, in the judgment of the Academy of Sciences of Turin, the most useful or most brilliant discovery, or shall have produced the most able work, in the physical and experimental sciences, natural history, pure and applied mathematics, chemistry, physiology and pathology, without excluding geology, history, geography and statistics.

Las Familias mas importantes del Reino Vegetal, especialmente las que son de interes en la Medecina, la Agricultura e Industria, o que estan representados en la Venezuela; por A. Ernst. 80 pp., 8vo. Caracas. 1881.

Second Report of the U. S. Entomological Commission for the years 1878 and 1879, relating to the Rocky Mountain Locust and the Western Cricket, by C. V. Riley, A. S. Packard, Jr., and C. Thomas. xviii, 322 and [80] pages, with many maps and plates.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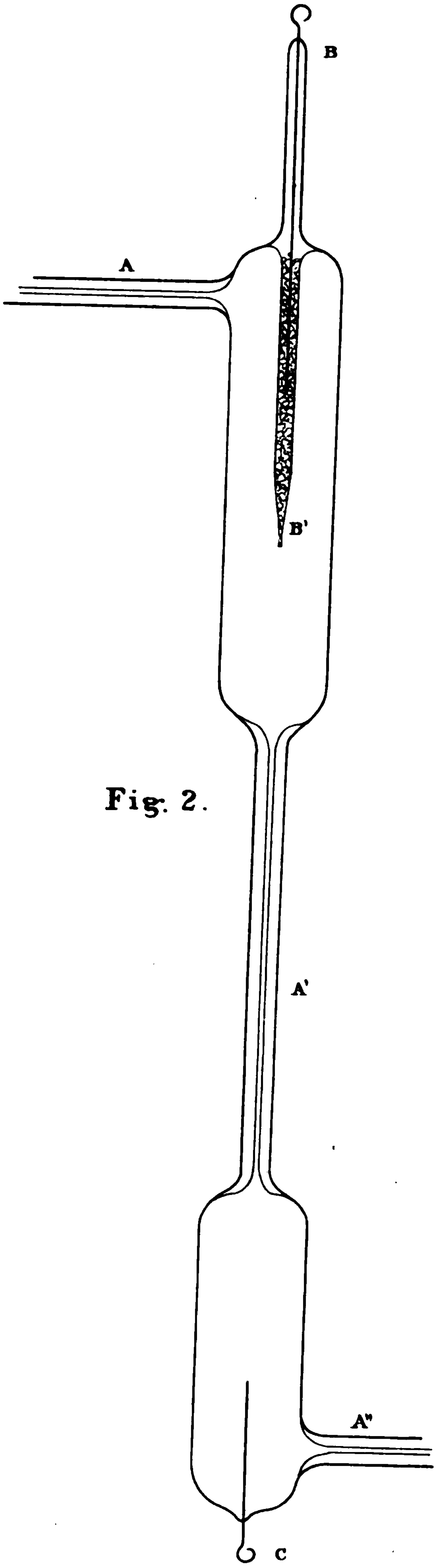
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Fig. 2.

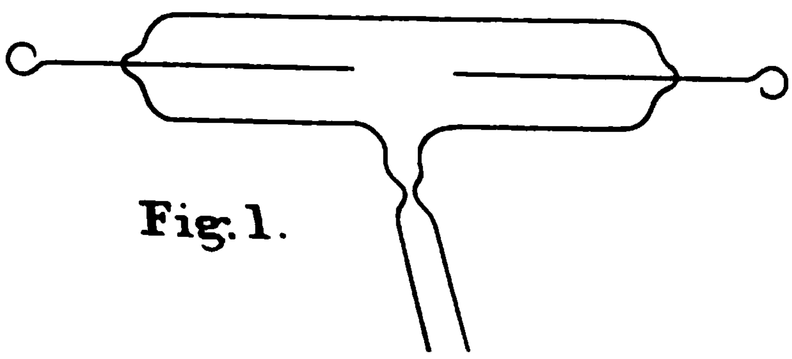


Fig. 1.

THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

Art. XXXV.—*On the Cause of the Arid Climate of the Western portion of the United States*; by Captain C. E. Dutton, U. S. A., U. S. Geological Survey.

Read before Section B, American Association for the Advancement of Science, Cincinnati Meeting, Aug. 18th, 1881.

Many questions arising in the study of western geology involve the consideration of the arid climate of the region, and I have frequently been led to inquire as to its cause. Arid climates are usually attributed to the passage of prevailing winds over high mountain chains. As they ascend the mountains upon the windward sides they are cooled by the expansion due to diminished barometric pressure, their capacity for moisture is reduced and an abundant precipitation takes place. Descending upon the leeward sides these changes are reversed; the air is heated, its capacity for moisture is increased, it becomes dry, and having been depleted of moisture is supposed to be incapable of yielding a copious supply to regions beyond. This explanation is no doubt good for some localities. Peru is a case in point and for that country it seems quite perfect. It is believed by many that it also explains the arid climate of the western half of the United States, and that the Sierra Nevada is the range which robs the winds of that region of the moisture which otherwise would make its vast expanse fertile. Reflection upon this case has led me to a different conclusion.

It is unquestionable that the Sierra Nevada abstracts a notable amount of moisture from the winds blowing from the

Pacific. Mr. B. B. Redding, the Land Agent of the Central Pacific Railroad, has kept for several years excellent records of the rainfall at many stations in California and Nevada, and informs me that along the main road from Sacramento to the summit pass of the Sierra, the annual rainfall increases at the rate of one inch for every one hundred feet of altitude. At the summit the mean annual precipitation exceeds ninety inches. It is not improbable that this large amount is considerably exceeded at numerous points along the crest of the range. It seems clear therefore that the winds which blow over the Sierra are to some notable extent depleted of moisture and the effect must be to at least aggravate the aridity of the regions lying immediately east of the range. But I think it can be made evident that this effect is relatively not great, and that the elevated region of the west would be on the whole very nearly as arid as it now is if the Sierra Nevada were obliterated as a mountain range. Nor can the other and lower ranges lying east of the Sierra affect the case materially, for surely more than ninety per cent of the rain and snow which fall upon them are reëvaporated *in loco* and the atmosphere ultimately suffers no material loss of moisture.

When the winds blow constantly from a cool to a warmer region they become warm and therefore dry; and if they have no opportunity to take up more moisture on the way the passage from a cool to a warm region is a sufficient cause of aridity. This is, I conceive, the state of affairs which determines the climate of the western mountain region. The winds blow constantly from the western quarters, being the "return-trades." Local winds and perhaps large cyclones occasionally turn the weathercock toward an easterly quarter, but the general drift of the great atmospheric ocean is ever from west to east.* This prevailing air drift comes from the Pacific and reaches the coast nearly or quite saturated with moisture. The quantity of moisture required for saturation is dependent chiefly upon temperature; and the temperature of the air as it reaches the coast is determined by oceanic conditions.

From the Aleutian Islands a coastwise ocean-current moves southward, having a breadth of 500 miles or more, and extending as far southward as the latitude of Cape St. Lucas. Off British Columbia and Alaska it may be regarded as a warm current relatively to the adjoining land. Off the Californias although its temperature rises notably with its southward movement it may be regarded as a relatively cool current. On the more northerly shores its effect is to make the climate of the adjacent coast warmer than it would otherwise be; and its

* This general statement requires some qualification when applied to southern Arizona and southern New Mexico, though it is in the main applicable even there.

effect on the more southerly shores is to make them cooler. Stated in another manner the relation is such that the temperatures of the land areas in the high latitudes are lower than those of the ocean, while in the low latitudes they are higher. In the high latitudes, therefore, the winds blowing from the Pacific are cooled by the land; in the low latitudes they are warmed by it. Hence the precipitation is copious in the former regions and meager in the latter. Between the two belts where these opposite effects are pronounced is a region where they shade into each other, and though this intermediate region cannot be marked out by distinct boundaries it may still be said to exist in latitudes lying within the valley of the Columbia River.

The cause of an arid climate thus indicated may be regarded as generally operative throughout the western mountain region; and it will no doubt appear upon full consideration to be much more potent and widely extended in its action than any or even all of the mountain ranges could be. It is, however, greatly modified by the intervention of local causes, which occasionally mask or obscure it. The precipitation in different portions of the region is highly irregular and several modifying causes can be indicated which, though they do not nullify the more general one here set forth, frequently become much more conspicuous in their effects. For instance, it is well-known that the heaviest rainfall in the United States, excepting possibly upon some mountain tops, occurs upon the coast of Oregon and Washington Territory. But as already indicated this is the locality where we find the neutral axis, so to speak, of the alleged causes favoring respectively humidity and aridity, and where their effects are at a minimum or even at zero. Moreover, the westerly winds saturated with moisture here strike the coastwise mountains, and are suddenly thrown upward several thousand feet before they have had time to feel the heating effect of the land which is here very slight; and the precipitation is thus very copious. Descending to lower levels inland they soon become dry and produce a sub-arid climate.

The most frequent variants of climate are the great differences of altitude in different portions of the west. The mountain tops and summits of the plateaus are always well watered, and in any given latitude the rainfall increases or diminishes at a fairly definite rate with the altitude. But the variation of rainfall with altitude is by no means a simple ratio. Between 4500 and 6000 feet the difference in rainfall is not great; between 6000 and 7500 feet it is very considerable; between 7500 and 9000 it is still greater.

Moreover the rainfall is greater *ceteris paribus* in high latitudes

than in low latitudes. In passing from the southern to the northern boundary, if we compare localities of equal altitudes along any given meridian, we shall find the rainfall steadily though perhaps not uniformly increasing. This is an obvious consequence of the theory suggested.

Although no very great effects upon the general condition of aridity are here attributed to the depletion of moisture by the passage of the winds over mountain ranges, it is still true, no doubt, that highly important local effects are thereby produced. The rainfall at the eastern base of the Sierra Nevada, and for two hundred miles east of it is most probably reduced very greatly by this cause. In the sink of the Humboldt River, the annual precipitation seldom reaches four inches, and may average not more than three inches. But as we pass eastward beyond the *wake* of this range, its effects become gradually less; and long before the Wasatch is reached they have become inconsiderable. Since the Sierra Nevada is the longest, highest and widest of the individualized ranges of the Rocky system, its local effect upon the humidity of the plains and valleys lying immediately under its lee is greater than that of any other. But the same kind of effect is preceptible in some other ranges.

The discussion of the causes of local variations in climate might be almost indefinitely extended. Nothing more is designed here than to advert to one general cause of aridity which prevails over the entire region, and which everywhere persists, though it is often obscured, sometimes reversed and sometimes reinforced by local causes.

ART. XXXVI.—*On additional Embryonic Forms of Trilobites from the Primordial Rocks of Troy, N. Y., with observations on the genera Olenellus, Paradoxides and Hydrocephalus;* by S. W. FORD.

AMONG the various species of Trilobites of the genus *Paradoxides* (abstracting those forms of which we know the thoracic structure but imperfectly or not at all), there may be distinguished two principal groups: One characterized by having the *second*, and rarely also the *first*, pleuron prolonged considerably beyond the succeeding ones; and the other by having all of the anterior pleura, as we proceed backward, decreasing or increasing in length, according to the species, in a regular manner. As examples of the former we may instance *Paradoxides spinosus* from the Bohemian Primordial, and *P. Bennettii* from that of Newfoundland (and the majority of the Bohemian species

might be included); and of the latter the most if not all of the British species, all of the Swedish, and the American *P. Harlani*. Why two species, so closely allied as are the *P. spinosus* and *P. Harlani*, should yet differ in the particulars mentioned, has all along been looked upon as a mystery; but there can be but little doubt that all who have seriously contemplated the matter, have regarded these differences as possessing a deep and peculiar significance.

The five known species of the American genus *Olenellus* admit of a similar grouping, and, if we confine ourselves to the adult forms alone, upon the ground of thoracic differences equally pronounced with those obtaining in the genus *Paradoxides*. Three of them, *O. Thompsoni*, *O. Vermontanus* and *O. Gilberti* have the *third* pleuron conspicuously prolonged beyond the others; while in *O. asaphoides* it forms, with those preceding and succeeding it, a regularly graduated series. The thorax of the fifth species, *O. Howelli*, has not been observed. These differences long since attracted the attention of paleontologists, and led at least one authority to exclude the *O. asaphoides* from the genus altogether,—apparently overlooking the fact that a similar course of reasoning would compel us to break up the genus *Paradoxides*.* But the facts now in hand show that *Olenellus asaphoides* is, beyond a doubt, a genuine *Olenellus*. As I shall have frequent occasion, in the course of this article, to refer to both the long and short ribbed forms spoken of, I shall designate them, whether referring to *Paradoxides* or *Olenellus*, as the *macropleural* and *brachypleural* types respectively. These

OLENELLUS ASAPHOIDES *Emm.*, SP.

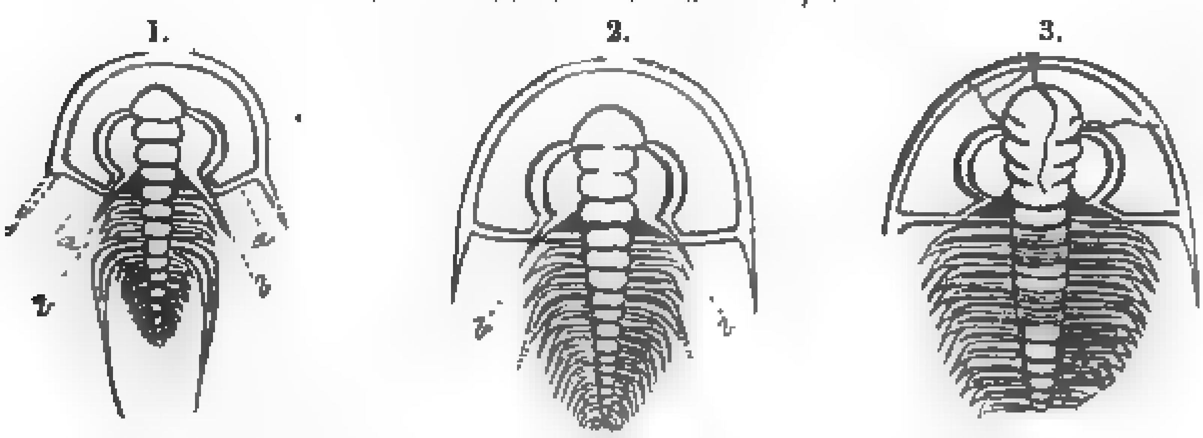


Fig. 1.—Embryonic form of *Olenellus asaphoides*, enlarged five diameters. Fig. 2.—Another specimen, representing a more advanced stage of development, enlarged four diameters. Fig. 3.—A still older specimen, the characters of which are all only those of the adult, enlarged two diameters.

terms, however, as will be seen further on, are not intended to be expressive of sharply defined or clearly distinct groups or sub-groups, but are here introduced merely for the sake of convenience.

* Thirteenth Regents' Report on the N. Y. State Cabinet, p. 119.

As the result of some recent researches in the Primordial beds of Troy, N. Y., I have obtained two specimens which afford a very satisfactory solution of the structural peculiarity noted above in the case of *Olenellus asaphoides*; besides offering a probable explanation of the brachypleurism observed in *Paradoxides*. They tend, moreover, to prove that the macropleural species of that genus should be regarded as typical. Both are young specimens of *Olenellus asaphoides* and unusually perfect. Their leading characters may be stated as follows:

Fig. 1 represents the younger and by far more important specimen, the place of which, in the embryonic series, is probably about mid-way between the forms represented by figures 2 and 3 of my former article (this Journal for April, 1877). There are either nine or ten body-segments, the last three or four being somewhat indistinct. The third pleuron is considerably larger and longer than the others, the points extending backward well beyond the limits of the thorax. All of the pleura have the characteristic groove of *Olenellus*. The posterior margin of the head is sharply geniculated at the sutures, throwing the genal spines notably forward upon the cephalic periphery, precisely as in *Paradoxides spinosus* Boeck and *P. pusillus* Barrande (see figs. 5 and 6). The interocular spines are prominent, and, although slightly damaged, can be seen to have reached nearly to the third body-segment. Moreover, these spines and the genal spines are still parallel with each other as in earlier embryonic life. The glabella is marked by three furrows besides the neck-furrow, all of which run entirely across it as in the known preceding stages of development.

Bohemian Paradoxides.

4. 5. 6.

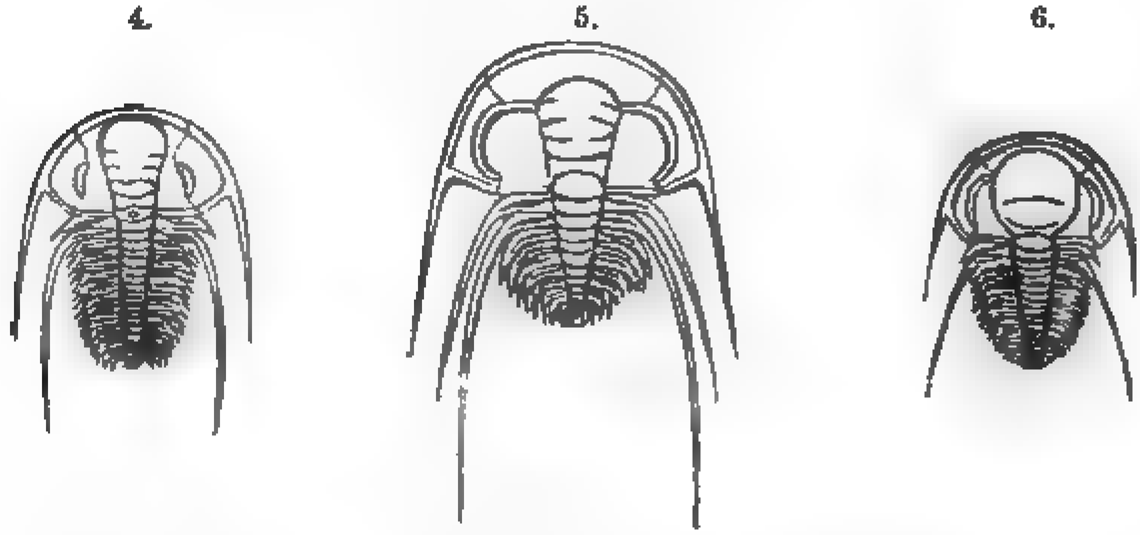


Fig. 4.—Young specimen of *Paradoxides spinosus* Boeck sp., twice enlarged.
Fig. 5.—Very perfect specimen of *P. pusillus* Barrande, enlarged 10 diameters.
Fig. 6.—Complete individual of *P. inflatus* Corda, enlarged 4 diameters. All after Barrande.

The length of the specimen, from the middle of the front margin to the extremity of the third pleuron, is 0·26 of an inch,

and the width of the head, exclusive of the posterior spines 0·14. The entire surface is plain, or without any trace whatever of ornamentation.

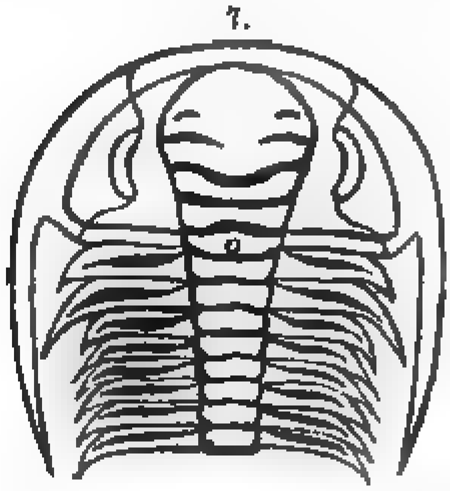
Fig. 2 represents the second and older specimen, the left-hand portion of which is partly restored in the drawing. The place of this specimen in the developmental series removes it a number of steps from the form just described, allying it much more nearly to those forms in which the metamorphoses are at an end. There are fourteen body-rings, and behind these a minute, rudely semi-circular plate (the pygidium), which I believe to have been the source of all the body-segments. The third pleuron is still conspicuously longer than the others; but its relative width, as compared with that in fig. 1, is much reduced, and its direction changed. How far backward it extended it is impossible to say, as both the right and left hand points are wanting; but the pleural furrows are here relatively much shorter than in fig. 1, and this fact strongly argues a corresponding abbreviation of the pleural points. The head forms rather less than a semi-circle, and has the posterior margin curved slightly forward; in other specimens, however, of the same size, and even smaller, the posterior margin is completely transverse, and hence the curving in the present instance is evidently an individual peculiarity. The interocular spines are very small, but are still attached to the fixed cheeks. The genal spines are slender, reach as far backward as the third pleura, and here form with the interocular spines a very appreciable angle. The glabella is somewhat crushed, but is seen to be furrowed nearly as in the adult. I cannot say with certainty whether all of the furrows in advance of the neck-furrow were separated on the median line or not. The surface is nearly smooth, but just beyond the eyes some obscure striation can be detected. The length of this specimen is 0·33 of an inch, and the breadth at the genal angles 0·24.

In fig. 3, which is an outline representation of fig. 5 of my former article, the third pleuron forms, with the others, a regular series, the interocular spines have disappeared, the head has assumed the form which it afterward retains, and the development is completed. Between this form and the preceding one, I have a considerable number of others, which leave no doubt as to their being fundamentally one and the same.

We learn, from the foregoing, the important fact, that the macropleural and brachypleural types under the genus *Olenellus* can in no wise be regarded as indicative of fixed or independent groups, *O. asaphoides* being macropleural in embryonic life and brachypleural in the adult; and this breaks down the dividing line between them. Now, according to a well-known

canon in Natural History, *O. asaphoides* must be regarded as higher in grade than its macropleural congeners; and this being true, we are naturally led to inquire whether the brachypleural forms under the genus *Paradoxides* are not also higher in grade than *their* macropleural congeners. Unfortunately, the direct evidence required to decide this question is wanting; but there are certain known facts having an important bearing upon it, and to these I shall now refer.

7.



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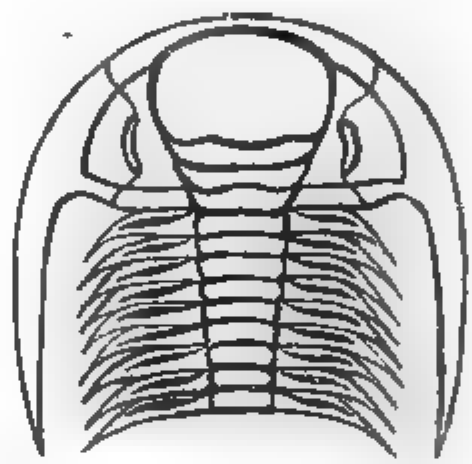


Fig. 7.—Head and first 8 body-segments of adult specimens of *Paradoxides spinosus* (macropleural), reduced two-thirds. Compare with fig. 4. Fig. 8.—Head and 9 forward pleura of adult of *P. Tessini* Brongn. (brachypleural), reduced one-half (after Angelin).

Among the macropleural *Paradoxides* described by Barrande, there are a number of species of which we lack either one or other of the growth extremes; some of them being known only by adult examples, and others only by forms which appear to be the young. I say *appear to be*, because, while I have myself no doubt that *P. pusillus* is a young Trilobite, there is nothing in the aspect of *P. inflatus* except its small size and excessively produced second pleura to indicate that it was not full grown. In the case of *P. spinosus* and *P. Bohemicus*, however, we know both the young and mature forms; and, as will be seen by figure 4, *P. spinosus*, in the young state, was not only pronouncedly, but even extravagantly, macropleural, the points of the second body-segment extending, like those of the third in the young of *O. asaphoides*, backward beyond the thorax; and in the young of *P. Bohemicus* this peculiarity is equally striking. But although this feature, in both of these species, was well-nigh obliterated in the adult, yet in neither was the process carried sufficiently far to render them brachypleural species. Nevertheless, it is not difficult to see that such a result might easily have been attained; and from what we now know of the history of *O. asaphoides*, coupled with the facts just stated, there is strong presumptive evidence that the brachypleural species of *Paradoxides* were macropleural in early

life. It is earnestly to be hoped that the British and Swedish *savans* will institute, at no distant day, new researches, with the view of reaching a clear and final settlement of this important question.

But by far the most interesting feature of the young specimen of *Olenellus asaphoides* first described yet remains to be particularly considered. I allude to the remarkable *Paradoxides*-like run of the outer portion of the posterior margin of the head, shown at *a a* in figure 1. This feature, though varying in the intensity of its expression in the several species, is, if we exclude one or two species which are in other respects abnormal, constant in the genus *Paradoxides*, and appears to have been especially emphasized in the forms of the macropleural section; but it is shown in none of the other species of the genus *Olenellus*, and even disappears altogether, as we have seen, in *O. asaphoides*, during embryonic life. After much study of the subject, I am convinced that we have here the exhibition of a character, afterwards lost, which in *Paradoxides* may be regarded as fixed. It is true, that in *O. Gilberti*,* the posterior margin is deeply emarginate in the vicinity of the postero-lateral angles; and this feature, as shown by the figures given in the Vermont Geological Reports, is sometimes present in *O. Vermontanus;* but the facial suture, in the former species, does not cut the posterior margin at the point of geniculation as in *Paradoxides*, but far within it; and this appears to hold good for the youngest specimen which Dr. White figures. It is evident, to my mind, that this character is not the same with that under discussion occurring in the young of *O. asaphoides;* and I believe that no one, who will take the trouble to examine the facts, will be likely to reach a different conclusion. The discovery, however, of still younger specimens of *O. Gilberti* is greatly to be desired, as they would doubtless serve to throw much light upon the whole question.

Now, if the foregoing interpretations be correct, *Olenellus asaphoides* must be regarded as higher in grade than any of the normal species of *Paradoxides;* and such I believe its history and structure alike declare it to be. The following additional facts and considerations appear to me to sustain this conclusion, and tend to clear up a number of points hitherto obscure connected with the subject.

Fig. 10 represents the plan of structure of the head of a Swedish *Paradoxides*, described by J. G. O. Linnarsson, in 1871, under the name of *P. Kjerulfi.*† The thorax in none of the examples figured is well preserved, but from the study of

*White, Rep. upon Geogr. and Geol. Explor. and Surv. west of the One Hundredth Meridian. Part I, vol. iv, Paleontology, p. 44, pl. 2 figs. 3 a–c.

† Oefversigt af Vetenskaps-Akademiens Förhandlingar, 1871, No. 6, Stockholm.

the head alone, no one thoroughly acquainted with Primordial Trilobites would hesitate to pronounce it a *Paradoxides;* and M. Linnarsson thus unquestioningly describes it. It is only when we come to compare it with such forms as *Olenellus*

9. 10. 11.

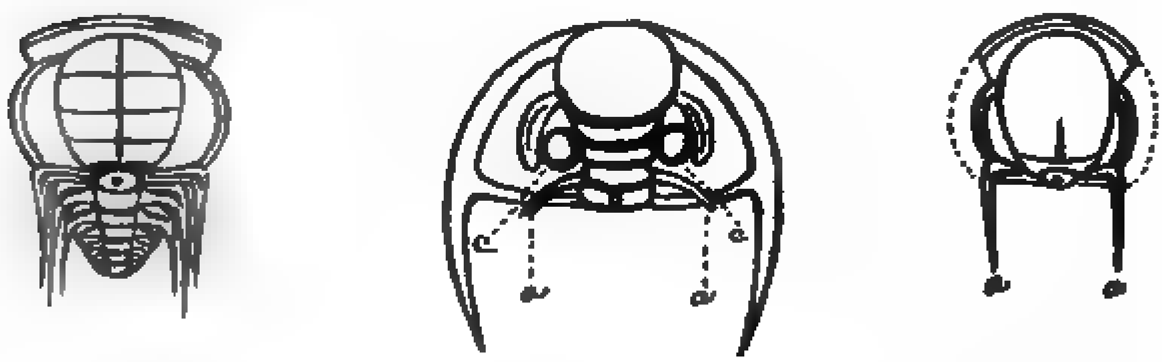


Fig. 9—Head (minus the free cheeks) and thorax of *Hydrocephalus Saturnoides* Barrande, enlarged 16 diameters. Fig. 10.—Plan of structure of the head of *Paradoxides Kjerulfi* Linnarsson, from the Swedish Primordial, nat. size. Fig. 11.—Head of *Hydrocephalus carens* Barrande (the free cheeks restored in outline), enlarged 6 diameters.

asaphoides and *O. Gilberti*, and especially with the young of the former, that the real difficulty arises. It differs from the other forms of the genus mainly in: (1) The possession of a pair of spinous processes extending from the neck-furrow backward across the posterior margin (fig. 10, *a a*); (2) The apparently firmly soldered facial sutures; and (3) The marked tumidity of the central portions of the fixed cheeks (fig. 10, *c c*). All of these characters, if we regard the spinous processes as the structural homologues of the interocular spines of *Olenellus asaphoides* -see figs. 1 and 2, *b b*—(and whether so or otherwise, I believe them to have been clearly functionally such) occur likewise in *O. asaphoides;* the first and third having here, however, only a transitory existence, while the second characterizes all the stages. *O. asaphoides* further shows its close relationship with the Swedish form in having all of its glabellar furrows, in early embryonic life, extending entirely across, instead of being interrupted, as in the more advanced and mature forms, on the median line. There can be scarcely a doubt that the figure of *Paradoxides Kjerulfi* above given represents a fully developed form, and that all of the characters which it exhibits were permanent in it.*

The above facts, taken in connection with those stated earlier, strongly argue that *Olenellus asaphoides* may be safely regarded as higher in grade than any known form of *Paradox-*

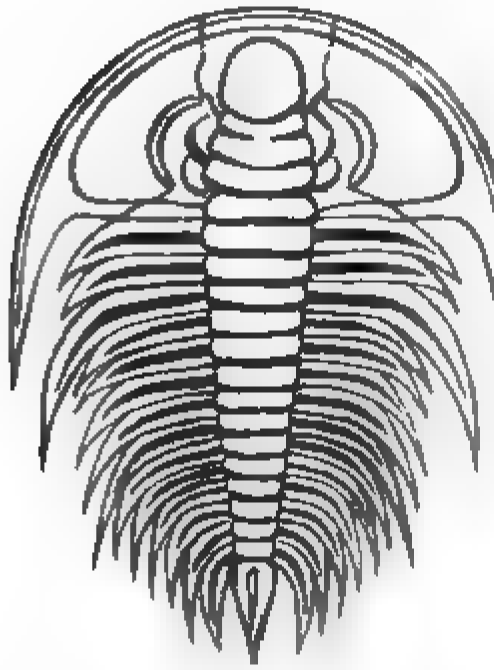
* It is worthy of remark, in this connection, that the solidity of the head-shield, due to the firm coalescence of the free and fixed cheeks in front of the eye, appears to have characterized all the known species of *Olenellus;* and that in one of them at least, *O. Thompsoni* (fig. 12), the central portions of the fixed cheeks, or interocular spaces, were notably inflated in adult life.

ides whatsoever; *P. Kjerulfi* being a normal species in so far as concerns the contour of the posterior margin of the head, but in other particulars one of the most widely divergent; and we here touch, it seems to me, the real core of the matter. The all-important question is, what is the precise nature of the relationship subsisting between these two species? We might, indeed, rest content with the deductions already arrived at and the inferences to which they lead; among which latter may be mentioned this: that if *O. asaphoides* has the superior zoological rank above accredited to it, it is probably a more recent form; and this fact accords well with the collective testimony of the other forms composing the local fauna (that of Troy, N. Y.,) to which it belongs; but are not the special relationships pointed out, one and all, the mere incidents of some profounder, all-embracing relationship? That such they are I cannot well doubt; and I am further compelled to add, that the study of the facts herein presented has produced in my mind a strong conviction that this relationship is probably deeper than an ordinal, a family, or even a generic one—in short, that it is genetic. And that this view of the case will ultimately prevail, there is, in my opinion, every reason to believe.

The weight of the evidence in this case may perhaps be better appreciated by a succinct restatement of it, and it amounts to this: that four out of five of the fixed characters of *P. Kjerulfi* above enumerated appear in the extreme young of *O. asaphoides* only to disappear; and in addition to this it loses during early life, as we have seen, its macropleurism. Had we but a single embryonic character linking this species with *Paradoxides* the case would be different, but we here have a whole congeries of such characters, clearly and unmistakably shown. It is true that, in his later writings (1879), M. Linnarsson refers to *P. Kjerulfi* as *Paradoxides* (*Olenellus*) *Kjerulfi;* but in 1877, shortly after the publication of my former article, we find him changing the title of *O. asaphoides*, as given by me, to *Paradoxides* ("*Olenellus*") *asaphoides;* that is to say, he endeavored to get over the difficulty by first turning *O. asaphoides* into a *Paradoxides*, and then turning *P. Kjerulfi* into an *Olenellus*, neither of which attempts have proved, however, at all satisfactory. I believe that, even if *O. asaphoides* be genetically related to *P. Kjerulfi*, we may yet with propriety consider it as generically distinct, and as such I still continue to look upon it. Nevertheless, I am free to own that, if we take into account the entire known range of structural characters under the genus *Paradoxides*, I see nothing, at present, in the finished form of *O. asaphoides*, that can be regarded as absolutely distinctive, except the segment furrow. If it be true that *O. asaphoides* has resulted from

the evolution of some *Paradoxides*-like form, then the line of descent probably extends backward through the macropleural section of the genus *Olenellus* to some such species as *Paradoxides Kjerulfi*, or perhaps to some still more divergent form of *Paradoxides*, with which we are as yet unacquainted.

12.



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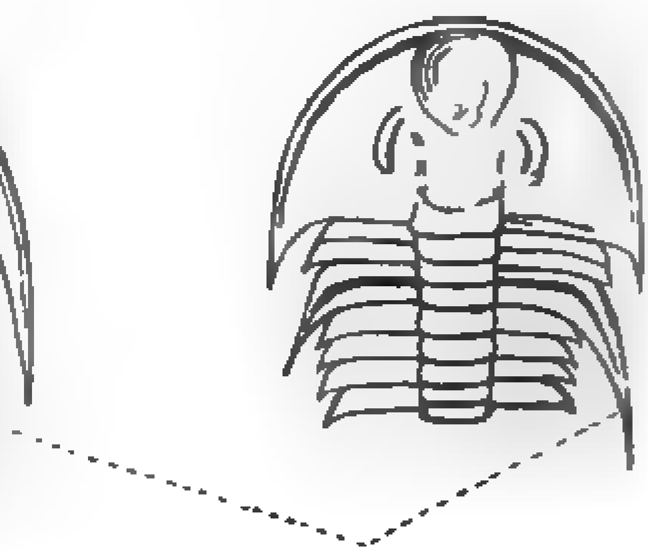


Fig. 12.—Adult specimen of *Olenellus Thompsoni* Hall, reduced one-half. **Fig. 13.**—Medium sized individual of *O. Vermontanus* Hall, natural size. Both after Hall.

Hydrocephalus is a still somewhat obscure genus occurring in the Bohemian Primordial; but, as long since pointed out by Barrande, one of the close allies of *Paradoxides*. It differs from *Paradoxides* mainly in the course of its facial sutures, and in the peculiar position of its genal spines; the former striking the posterior margin, according to Barrande, in such a way as to leave the latter attached to the fixed cheeks (see fig. 11, *a a*). Barrande considers the head to have had the form shown in the figure referred to, but the free cheeks have never been observed. Hence a doubt may well exist as to whether what he here calls the genal spines are truly such. M. Linnarsson considers them the probable homologues of the spines of the fixed cheeks of his *Paradoxides* ("*Olenellus*") *Kjerulfi*, and the interocular spines of *Olenellus asaphoides*. It is possible that his view is the correct one, and, if so, the head, when perfect, probably had much the form of that in fig. 6. At present, however, I do not share this opinion, believing them to be altogether peculiar. The discovery of a perfect specimen is greatly to be desired. *Anopolenus* (Salter) is another of the close allies of *Paradoxides*; and in the *P. expectans* of Barrande (Syst. Silur., etc., vol. i, supplt., pl. 3, figs. 33–35, and pl. 14, fig. 35) we have a type so closely resembling it as to strongly

suggest for them a genetic kinship (see Hicks, Quart. Jour. Geol. Soc., vol. xxviii, pl. 7, figs. 1–11). Salter states that, in the British Primordial, the genera *Paradoxides*, *Anopolenus* and *Olenus* follow each other in regular order—first *Paradoxides*, then *Anopolenus* and lastly *Olenus;* and in America we appear to have a like succession—first *Paradoxides*, then *Olenellus*, and lastly the Olenoid types of the western States.

The remarkable intersection of differential characters observed in the embryonic forms of *Olenellus asaphoides*, and the transformations there noted, appear to me to point to the *Embryo* as the principal theatre of organic evolution in general; and they strongly suggest, to my mind, the operation of profounder laws than any, hitherto assumed, as having effectively directed its course. It seems well-nigh absurd to ascribe such effects to natural selection, or the influence of environmental conditions, although such influences have, no doubt, to some extent, modified the total result. So far, however, as we are enabled to judge, the conditions of existence in Primordial times were remarkably uniform, and the "struggle for existence" was probably less a struggle then than now. And if it be true that the transformations wrought were mainly completed in embryonic life, and that, too, largely independent of external influences, it is no wonder that the great wealth of Silurian life still lies before us practically a sealed book, for it is only in exceptional instances that we may hope to be permitted to study the embryology of animal forms long extinct.

In the preparation of this paper I have all along felt my own unworthiness to deal in a befitting manner with the difficult problems which its subject matter presents; while as concerns the principal conclusion reached, or that touching the question of genesis, I should prefer to be understood as expressing in it rather my present convictions than my mature or final judgments. Nevertheless, I believe it to have a veritable basis in the known facts, and that its presentation is fully warranted by them; but those better qualified to judge may decide differently, and thus the real truth of the matter, even if I have missed it, will be likely, sooner or later, to come out. I have thought it well to assume but little, and to proceed according to the light of the evidence.

June 13th, 1881.

ART. XXXVII.—*Observations of Comet b*, 1881, *made at the Washburn Observatory, University of Wisconsin, Madison*; by EDWARD S. HOLDEN.*

THE following observations of the bright comet of 1881 have been made at the Washburn Observatory, with the Clark equatorial of 15·5 inches aperture, mostly with an eye-piece magnifying 145 diameters, having a field of 25′·5.

The accompanying engravings first appeared in *Science* of July 23 and August 6, and have been kindly furnished by the editor. In these (except in the case of the drawing of July 11), the darker the shading the brighter the corresponding part of the comet.

The Washburn Observatory is $0^h\ 49^m\ 25^s{\cdot}8$ west of Washington. The *times* are, however, Chicago mean times, or correspond to a meridian $0^h\ 7^m\ 11^s{\cdot}1$ east of our own, that is $0^h\ 42^m\ 14^s{\cdot}7$ west of Washington.

1.

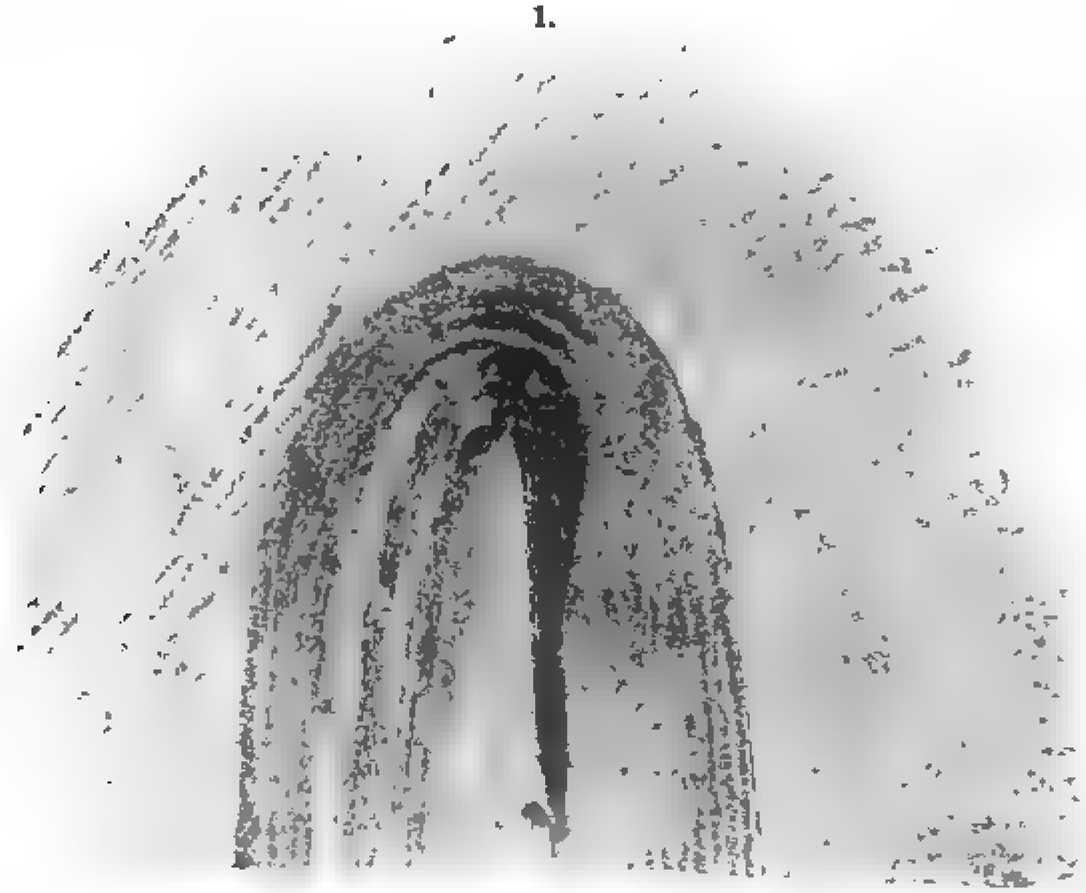
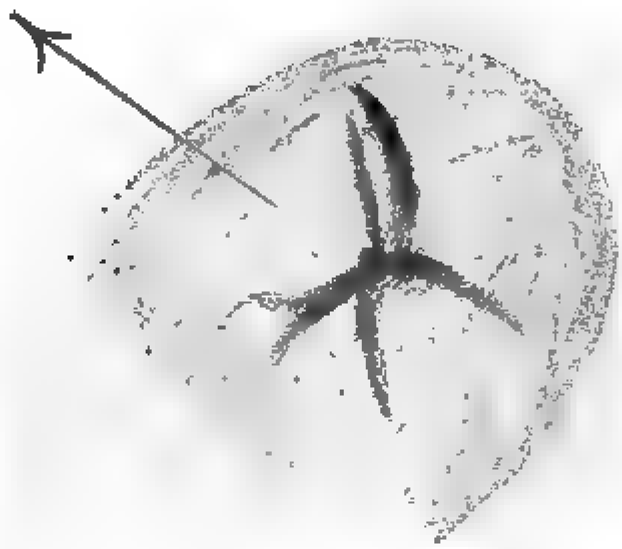


Figure 1; June 24, 14^h m. t.—This figure is intended to show the whole structure of the head of the comet, with its envelopes. There is a star within the tail.

Figure 2; June 25, 10^h m. t.—Sky hazy and outlines of the comet not well seen. The drawing shows only the structure of

* For the cuts illustrating Professor Holden's paper, this Journal is indebted to Mr. John Michels, editor of "Science," in whose pages the above illustrations were first published.

2.

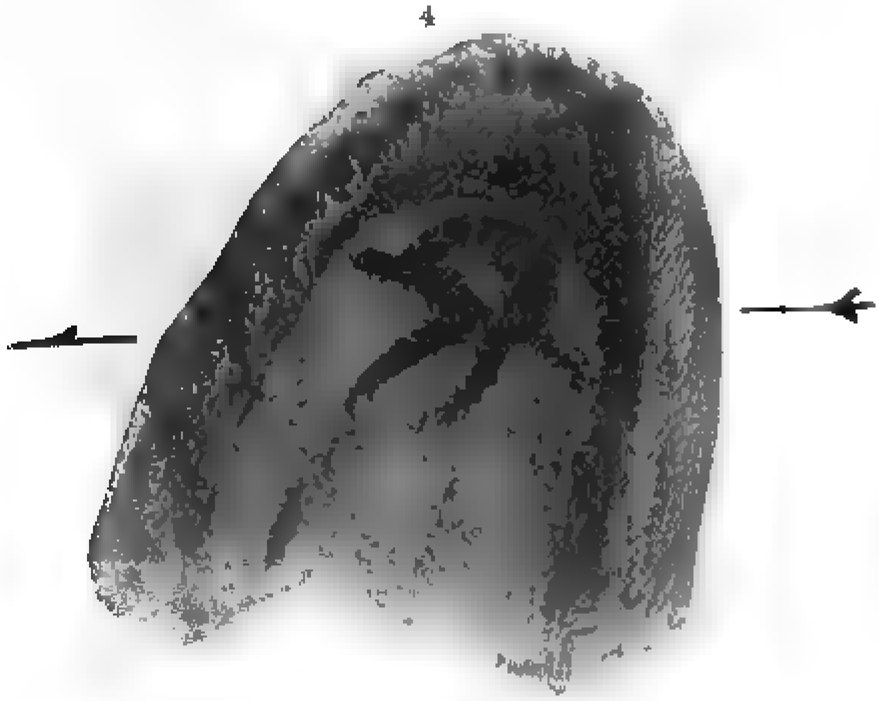


June 25.

3.

June 26.

4



June 27

the head. The nucleus is not round and is eccentric in the envelopes. The arrow shows the parallel.

Figure 3; June 26, 11^h 22^m m. t.—Hazy and cloudy.

Figure 4; June 27, 13^h m. t.

5.

6.

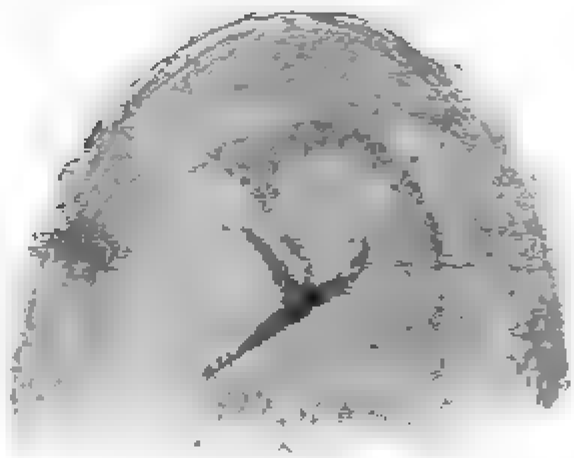


Figure 5; June 28, 10^h m. t.

Figure 6; June 29, 9^h 30^m m. t.—I was absent from Madison till July 8.

7.

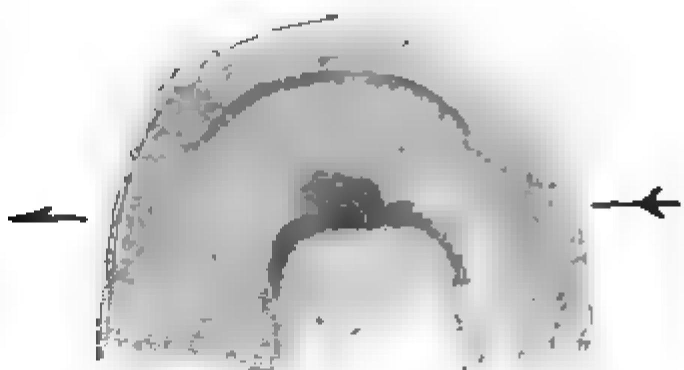


Figure 7; July 8, 10^h 35^m m. t.—Moonlight. The nucleus is not double. There is a dark narrow channel between the following side of the nucleus and the envelopes, as in the figure.

8.

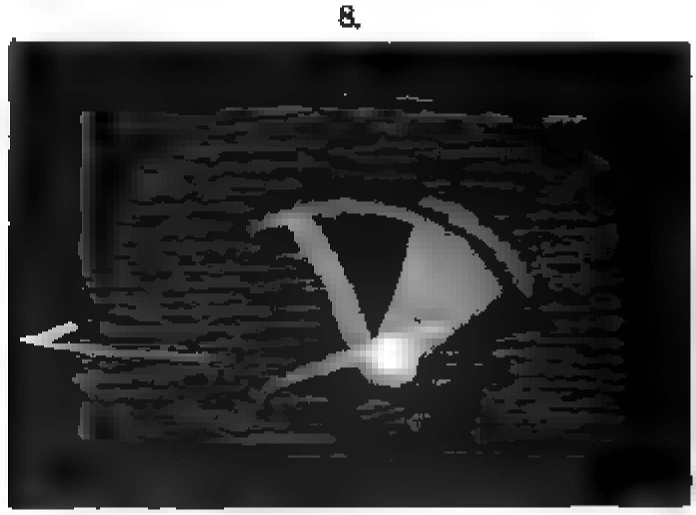
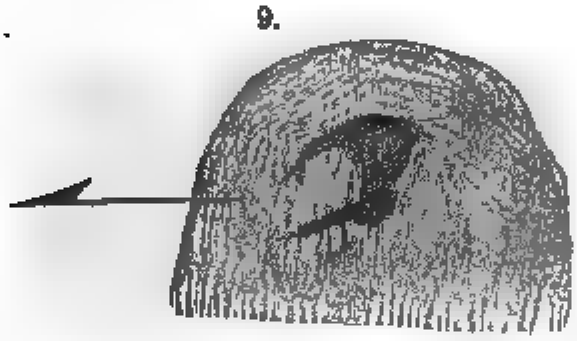


Figure 8; July 11, 9^h 30^m, m. t.—Strong moonlight and twilight. This cut gives bright portions of the comet by white lines.

Figure 9; July 13, 9^h 30^m m. t.
Figure 10; July 14, 10^h 20^m m. t.—Moonlight.
Figure 11; July 17, 10^h 45^m m. t.
Figure 12; July 18, 9^h 30^m–11^h 0^m m. t.—The nucleus is double (it has not been previously), $p=275°$, $s=1''{\cdot}5$, with a dark space between the parts.

9.



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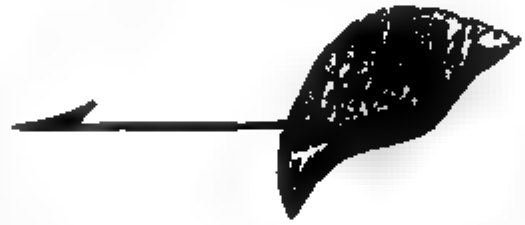

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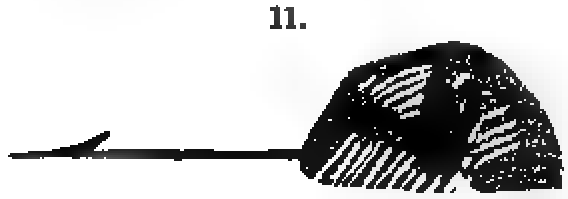

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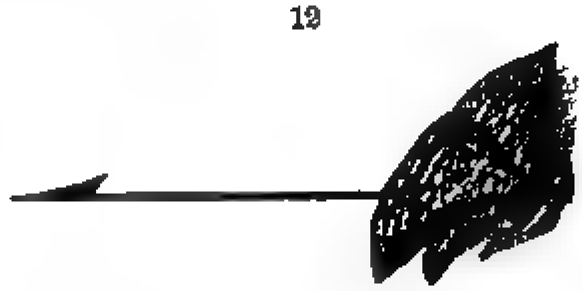

July 19; 9^h 45^m m. t.—Appearances same as last night, but fainter. The nucleus is elongated in $p=280°\pm$. The second nucleus is in $p=270°$ $s=1''$ to $2''$.

July 24; 9^h 35^m m. t.—The nucleus is double, $p=225°$ (4) $s=2''{\cdot}62$ (3). The diameter of the principal nucleus in $p=135°$, is $1''{\cdot}68$ (2).

The micrometer measures by Mr. Burnham.

July 26; 9^h 3^m m. t. The nucleus is round.

July 27; 10^h 10^m m. t.—The nucleus seems elongated in $p=250°$, but I am not sure.

After this date the comet was examined on several occasions without finding any peculiarity worthy of mention. It is to be noted that there is no doubt whatever as to the fact that the nucleus was double on July 18, July 19 and July 24. I am almost equally positive that it was not double on the other dates specified.

It appears to me that these observations are of interest in connection with those of Prof. O. Stone at the Cincinnati Observatory, and of Mr. Wendell at the Harvard College Observatory.

Washburn Observatory, Madison, Wisconsin, August 25, 1881.

ART. XXXVIII.—*On the thickness of the Ice-sheet at any Latitude*; by W. J. McGEE.*

1. ESTIMATES OF THICKNESS.

First preliminary estimate.—It was shown in Part I of this paper that the accumulation of glacier ice is dependent on precipitation; and in a general way it may be considered proportional therewith. It may also be assumed that the precipitation, and hence of course the accumulation of ice, is proportional to the vapor-tension. If then the thickness at any latitude is known, that at all other latitudes can be readily computed.

Professor Dana has shown † that the thickness of the Quaternary ice-sheet over the Canadian highlands (about N. lat. 48° to 50°) must have been at least 12,000 feet. As this accumulation took place under conditions less favorable than those considered in the present discussion, it may be assumed that a thickness of three miles might obtain at lat. 40°. The thickness at each latitude from 40° to the pole would accordingly be as represented in table XVII. The data forming the basis of the computation are derived from sources previously enumerated.

TABLE XVII.

Greatest thickness of Ice-field from lat. 40° to the Pole.

Latitude.	Temperature.	Vapor-tension.	Thickness of ice.
40°	+56·5° F.	0·457 in.	3·000 miles.
50	41·7	·264	1·733
60	30·2	·168	1·103
70	16·0	·090	·591
80	6·8	·059	·387
90	2·3	·048	·315

Second preliminary estimate.—It would doubtless be more satisfactory to base estimates upon the present accumulation of ice over polar regions, if the quantity were at all definitely known. The uncertainty regarding the exact amount is so great, however, especially in arctic regions, that any such estimate will serve only as a check on that already made.

It may be almost arbitrarily assumed that, if the land ice existing on the zone bounded by the eightieth parallel were uniformly distributed, it would form a sheet fifty feet in thickness. Now too little aqueous vapor is conveyed into arctic regions to permit the accumulation of sufficient ice to form an effective condenser. It is probable that, in consequence of this imperfection

* This article is from Mr. McGee's paper on "Maximum Synchronous Glaciation," making 65 pages of the Proceedings of the American Association for the Advancement of Science, vol. xxix, 1880.

† This Journal, March, 1873.

of the arctic condensing apparatus, enough moisture is not congealed, but allowed to fall as rain and thus to melt a portion of the ice, to reduce the accumulation which should take place by fully two-thirds. Were it not for this the accumulation might reach 150 feet on an average, and 300 feet near the margin. The corresponding maximum thickness when the ice extended ten degrees farther from the pole would be about 400 feet. These estimates enable us to institute a comparison with the antarctic ice-sheet.

Only about one-seventh of the seventieth parallel of north latitude is so free from land as to present no obstruction to the carrying in of vapor from more southerly regions. In the southern hemisphere, on the other hand, the whole parallel is practically open to the introduction of vapor from the adjacent temperate zone. The accumulation here ought accordingly to be seven times as great as in arctic regions, or 2,800 feet near the margin. It will probably not be objected that these estimates are too low, as they have purposely been made as large as seems at all consistent with the present condition of polar regions. It has already been shown that the present accumulation in these regions is probably about as great as ever can have existed.

Accepting the largest of these estimates as representing the greatest possible thickness of the ice-cap at lat. 70°, and computing the thickness at other latitudes as in table XVII, the respective values are found to be as follows:—

Lat.	40°	14217	feet,	=	2·693	miles.
"	50	8213	"	=	1·555	"
"	60	5226	"	=	·990	"
"	70	2800	"	=	·530	"
"	80	1835	"	=	·348	"
"	90	1493	"	=	·283	"

The approximate correspondence between the two estimates is apparent.

Final estimate.—It may be assumed that, in a hemisphere with parallel isotherms and isobars, all vapor is precipitated nearer the poles than where it is formed. Two factors (perhaps unequal), tending to produce opposite results in the final computation, will be disregarded. These factors are (1) the elevation of temperature outside the ice-field illustrated by table VI, and (2) the less frequent saturation of the atmosphere in frigid climates. As shown by the tables of Section II, when the ice-sheet reached any latitude the vapor which had previously been borne polar-ward would be precipitated near the margin of the sheet, mainly in the form of snow. The precipitation would hence be greater than the normal, at the border of the ice, in the ratio of $p : p + \frac{po}{n}$, where p denotes normal precipitation, o

area of zone bounded by margin of ice, and n area of hemisphere. Table XVIII has been computed in accordance with this ratio.

TABLE XVIII.

Maximum thickness of Ice-cap.

Latitude.	Temperature. —Dove.	Vapor-tension.	Value of $p+\frac{p\,o}{n}$.	Thickness of Ice-cap. Feet.	Miles.
10°	+79·9° F.	1·020 in.	1·863	55,871	10·582
20	77·4	·940	1·559	46,753	8·855
30	69·8	·728	1·092	32,749	6·203
40	56·5	·457	·620	18,594	3·422
50	41·7	·264	·326	9,777	1·852
60	30·2	·168	·191	5,728	1·085
70	16·0	·090	·095	2,800	·530
80	6·8	·059	·060	1,799	·341
90	2·3	·048	·048	1,440	·273

It is almost needless to reiterate the proposition already demonstrated, that vapor could not be borne far enough within the margin of the ice to affect materially the above results, without seriously deranging the sequence of phenomena to which the ice owes its origin and conservation.

The suggestion that the property of flowing might enable the ice to assume a uniform depth may be anticipated by mentioning that the polar slope above given is less than one-tenth of that requisite, according to Hopkins's experiments, to produce the slightest motion.

2. COMPARISON WITH THE ICE-CAP THEORY.

Concomitants of the theory.—The ice-cap theory seems to have been framed chiefly to account for the equatorial motion of the Quaternary glaciers. Now, to be consistent with itself, the theory requires that the assumed thickness of the cap shall be sufficient to form a slope down which ice will flow by gravitation alone. Hopkins found that ice barely moves on a slope of one degree; and there is no evidence that existing glaciers move on a less slope. To form such a slope from lat. 40° to the pole, the polar thickness of the ice would have to be 60 miles—the "twenty leagues" of Adhemar. If, with the same mean thickness, it extended only to lat. 45°, the content of the cap would be 575,000,000 cubic miles, equal (the density of ice to water being as ·92 to 1) to 529,000,000 cubic miles of water. But taking the water-area of the globe at 145,000,000 square miles, and the mean depth at 12,144 feet, or 2·3 miles,* we find that

* Sir Wyville Thompson says: "It seems now to be thoroughly established by lines of trustworthy soundings which have been run in all directions, that the average depth of the ocean is a little over 2,000 fathoms." This Journal, vol. xvi, (1878), p. 351. Dr. Krümmel estimates the mean depth at 1877 fathoms. See note in Popular Science Monthly, vol. xvi, Dec. 1879, p. 287.

all the water of the globe amounts to only 335,500,000 cubic miles or but little more than three-fifths of that required to form the assumed ice-cap.

If the above estimate seems too large, let it be reduced by seven-eighths, which will bring it well within the bounds prescribed by more moderate advocates of the theory; but even then it is too large to be admissible; for it would require one-fifth of the water of the globe to form even the smaller ice-cap. But diminishing the water of the globe one-fifth would diminish the water-covered area by a considerably larger fraction; for the sea bottom does not descend uniformly to the deeper abysses. The slope is, usually, gentle for a considerable distance from the shore, and then steep and precipitous to the abyssal depths. Reducing the water one-fifth would therefore reduce the area covered by it one-third. Suppose now the ice-cap be around the south pole: The diminution caused by the removal of so much water, and the further diminution resulting from the displacement of the earth's center of gravity, would drain nearly all the water from the northern hemisphere. But the consequent stoppage of marine circulation and of the formation of aqueous vapor would, as shown in Section I, so increase the diurnal and annual thermometrical range as to render the hemisphere uninhabitable for existing organisms.

Relative mass of the two ice-caps.—Assuming the ice-field tabulated above to be of uniform thickness for five degrees on each side of the parallels given, and to extend to lat. 45°, its mean depth would be 1·356 miles. Its mass would therefore be only $\frac{1}{15}$ of the larger or little over $\frac{1}{2}$ of the smaller of the ice-caps considered in the preceding paragraphs. It should be borne in mind, too, that this is the *maximum synchronous accumulation* under more favorable conditions than would be likely to obtain in nature. The consequent displacement of the earth's center of gravity has accordingly not been computed.

Conclusion.—It seems quite safe to affirm that in any extensive polar ice-field the thickness will decrease from near the margin toward the pole, where the attenuation will be greatest. It may accordingly be concluded that a sufficient accumulation of polar ice to displace seriously the earth's center of gravity or influence the motion of middle-latitude glaciers, can never have taken place in this hemisphere.

Art. XXXIX.—*Address of Sir John Lubbock, President of the British Association at York.*

* * The connection of the British Association with the City of York does not depend merely on the fact that our first meeting was held here. It originated in a letter addressed by Sir David Brewster to Professor Phillips, as Secretary to your York Philosophical Society, by whom the idea was warmly taken up. The first meeting was held on September 26, 1831, the chair being taken by Lord Milton, who delivered an address, after which Mr. William Vernon Harcourt, Chairman of the Committee of Management, submitted to the meeting a code of rules which had been so maturely considered, and so wisely framed, that they have remained substantially the same down to the present day.

Of those who organized and took part in that first meeting, few, alas, remain. Brewster and Phillips, Harcourt and Lord Milton, Lyell and Murchison, all have passed away, but their memories live among us. Some few, indeed, of those present at our first meeting we rejoice to see here to-day, including one of the five members constituting the original organizing Committee, our venerable Vice-President, Archdeacon Creyke.

The constitution and objects of the Association were so ably described by Mr. Spottiswoode, at Dublin, and are so well known to you, that I will not dwell on them this evening. The excellent President of the Royal Society, in the same address, suggested that the past history of the Association would form an appropriate theme for the present meeting. The history of the Association, however, is really the history of science, and I long shrunk from the attempt to give even a panoramic survey of a subject so vast and so difficult; nor should I have ventured to make any such attempt, but that I knew I could rely on the assistance of friends in every department of science.

Certainly, however, this is an opportunity on which it may be well for us to consider what have been the principal scientific results of the last half-century, dwelling especially on those with which this Association is more directly concerned, either as being the work of our own members, or as having been made known at our meetings. I have, moreover, especially taken those discoveries which the Royal Society has deemed worthy of a medal. It is of course impossible within the limits of a single address to do more than allude to a few of these, and that very briefly. In dealing with so large a subject I first hoped that I might take our annual volumes as a text-book. This, however, I at once found to be quite impossible. For instance, the first volume commences with a

Report on Astronomy by Sir G. Airy; I may be pardoned, I trust, for expressing my pleasure at finding that the second was one by my father, on the Tides, prepared, like the preceding, at the request of the Council; then comes one on Meteorology by Forbes, Radiant Heat by Baden Powell, Optics by Brewster, Mineralogy by Whewell, and so on. My best course will therefore be to take our different Sections one by one, and endeavor to bring before you a few of the principal results which have been obtained in each department.

The Biological Section is that with which I have been most intimately associated, and with which it is, perhaps, natural that I should begin.

Fifty years ago it was the general opinion that animals and plants came into existence just as we now see them. We took pleasure in their beauty; their adaptation to their habits and mode of life in many cases could not be overlooked or misunderstood. Nevertheless, the book of Nature was like some richly illuminated missal, written in an unknown tongue; the graceful forms of the letters, the beauty of the coloring, excited our wonder and admiration; but of the true meaning little was known to us; indeed we scarcely realized that there was any meaning to decipher. Now glimpses of the truth are gradually revealing themselves; we perceive that there is a reason—and in many cases we know what that reason is—for every difference in form, in size and in color; for every bone and every feather, almost for every hair. Moreover, each problem which is solved opens out vistas, as it were, of others perhaps even more interesting. With this great change the name of our illustrious countryman, Darwin, is intimately associated, and the year 1859 will always be memorable in science as having produced his great work on "The Origin of Species." In the previous year he and Wallace had published short papers, in which they clearly state the theory of natural selection, at which they had simultaneously and independently arrived. We cannot wonder that Darwin's views should have at first excited great opposition. Nevertheless from the first they met with powerful support, especially, in this country, from Hooker, Huxley and Herbert Spencer. The theory is based on four axioms:—

"1. That no two animals or plants in nature are identical in all respects.

"2. That the offspring tend to inherit the peculiarities of their parents.

"3. That of those which come into existence, only a small number reach maturity.

"4. That those, which are, on the whole, best adapted to the circumstances in which they are placed, are most likely to leave descendants."

Darwin commenced his work by discussing the causes and extent of variability in animals, and the origin of domestic varieties; he showed the impossibility of distinguishing between varieties and species, and pointed out the wide differences which man has produced in some cases—as, for instance, in our domestic pigeons, all unquestionably descended from a common stock. He dwelt on the struggle for existence (which has since become a household word), and which, inevitably resulting in the survival of the fittest, tends gradually to adapt any race of animals to the conditions in which it occurs.

While thus, however, showing the great importance of natural selection, he attributed to it no exclusive influence, but fully admitted that other causes—the use and disuse of organs, sexual selection, etc.—had to be taken into consideration. Passing on to the difficulties of his theory he accounted for the absence of intermediate varieties between species, to a great extent, by the imperfection of the geological record. Here, however, I must observe that, as I have elsewhere remarked, those who rely on the absence of links between different species really argue in a vicious circle, because wherever such links do exist they regard the whole chain as a single species. The dog and jackal, for instance, are now regarded as two species, but if a series of links were discovered between them they would be united into one. Hence in this sense there never can be links between any two species, because as soon as the links are discovered the species are united. Every variable species consists, in fact, of a number of closely connected links.

But if the geological record be imperfect, it is still very instructive. The further paleontology has progressed the more it has tended to fill up the gaps between existing groups and species, while the careful study of living forms has brought into prominence the variations dependent on food, climate, habitat, and other conditions, and shown that many species long supposed to be absolutely distinct are so closely linked together by intermediate forms that it is difficult to draw a satisfactory line between them. Thus the European and American bisons are connected by the *Bison priscus* of Prehistoric Europe; the grizzly bear and the brown bear, as Busk has shown, are apparently the modern representatives of the cave bear; Flower has pointed out the paleontological evidence of gradual modification of animal forms in the Artiodactyles; while among the Invertebrata, Carpenter and Williamson have proved that it is almost impossible to divide the Foraminifera into well-marked species; and, lastly, among plants, there are large genera, as, for instance, *Rubus* and *Hieracium*, with reference to the species of which no two botanists are agreed.

The principles of classification point also in the same direc-

tion, and are based more and more on the theory of descent. Biologists endeavor to arrange animals on what is called the "natural system." No one now places whales among fish, bats among birds, or shrews with mice, notwithstanding their external similarity; and Darwin maintained that "community of descent was the hidden bond which naturalists had been unconsciously seeking." How else, indeed, can we explain the fact that the framework of bones is so similar in the arm of a man, the wing of a bat, the fore-leg of a horse, and the fin of a porpoise—that the neck of a giraffe and that of an elephant contain the same number of vertebræ?

Strong evidence is, moreover, afforded by embryology; by the presence of rudimentary organs and transient characters, as, for instance, the existence in the calf of certain teeth which never cut the gums, the shrivelled and useless wings of some beetles, the presence of a series of arteries in the embryos of the higher Vertebrata exactly similar to those which supply the gills in fishes, even the spots on the young blackbird, the stripes on the lion's cub; these, and innumerable other facts of the same character, appear to be incompatible with the idea that each species was specially and independently created; and to prove, on the contrary, that the embryonic stages of species show us more or less clearly the structure of their ancestors.

Darwin's views, however, are still much misunderstood. I believe there are thousands who consider that according to his theory a sheep might turn into a cow, or a zebra into a horse. No one would more confidently withstand any such hypothesis, his view being, of course, not that the one could be changed into the other, but that both are descended from a common ancestor.

No one, at any rate, will question the immense impulse which Darwin has given to the study of natural history, the number of new views he has opened up, and the additional interest which he has aroused in, and contributed to, Biology. When we were young we knew that the leopard had spots, the tiger was striped, and the lion tawny; but why this was so it did not occur to us to ask; and if we had asked no one would have answered. Now we see at a glance that the stripes of the tiger have reference to its life among jungle-grasses; the lion is sandy, like the desert; while the markings of the leopard resemble spots of sunshine glancing through the leaves. Again, Wallace in his charming essays on natural selection has shown how the same philosophy may be applied even to birds' nests —how, for instance, open nests have led to the dull color of hen birds; the only British exception being the kingfisher, which, as we know, nests in river-banks. Lower still, among insects, Weismann has taught us that even the markings of

caterpillars are full of interesting lessons; while, in other cases, specially among butterflies, Bates has made known to us the curious phenomena of mimicry.

The science of embryology may almost be said to have been created in the last half-century. Fifty years ago it was a very general opinion that animals which are unlike when mature, were dissimilar from the beginning. It is to Von Baer, the discoverer of the mammalian ovum, that we owe the great generalization that the development of the egg is in the main a progress from the general to the special, that zoological affinity is the expression of similarity of development, and that the different great types of animal structure are the result of different modes of development—in fact, that embryology is the key to the laws of animal development.

Thus the young of existing species resemble in many cases the mature forms which flourished in ancient times. Huxley has traced up the genealogy of the horse to the Miocene Anchitherium, and his views have since been remarkably confirmed by Marsh's discovery of the Pliohippus, Protohippus, Miohippus and Mesohippus, leading down from the Eohippus of the early Tertiary strata. In the same way Gaudry has called attention to the fact that just as the individual stag gradually acquires more and more complex antlers: having at first only a single prong, in the next year two points, in the following three, and so on; so the genus, as a whole, in Middle Miocene times, had two pronged horns; in the Upper Miocene, three; and that it is not till the Upper Pliocene that we find any species with the magnificent antlers of our modern deer. It seems to be now generally admitted that birds have come down to us through the Dinosaurians, and, as Huxley has shown, the profound break once supposed to exist between birds and reptiles has been bridged over by the discovery of reptilian birds and bird-like reptiles; so that, in fact, birds are modified reptiles. Again, the remarkable genus Peripatus, so well studied by Moseley, tends to connect the annulose and articulate types. Again, the structural resemblances between Amphioxus and the Ascidians had been pointed out by Goodsir; and Kowalevsky in 1866 showed that these were not mere analogies, but indicated a real affinity. These observations, in the words of Allen Thomson, "have produced a change little short of revolutionary in embryological and zoological views, leading as they do to the support of the hypothesis that the Ascidian is an earlier stage in the phylogenetic history of the mammal and other vertebrates."

The larval forms which occur in so many groups, and of which the Insects afford us the most familiar examples, are, in the words of Quatrefages, embryos, which lead an independent

life. In such cases as these external conditions act upon the larvæ as they do upon the mature form; hence we have two classes of changes, adaptational or adaptive, and developmental. These and many other facts must be taken into consideration; nevertheless naturalists are now generally agreed that embryological characters are of high value as guides in classification, and it may, I think, be regarded as well-established that, just as the contents and sequence of rocks teach us the past history of the earth, so is the gradual development of the species indicated b the structure of the embryo and its developmental changes.

When the supporters of Darwin are told that his theory is incredible, they may fairly ask why it is impossible that a species in the course of hundreds of thousands of years should have passed through changes which occupy only a few days or weeks in the life-history of each individual?

The phenomena of yolk-segmentation, first observed by Prevost and Dumas, are now known to be in some form or other invariably the precursors of embryonic development; while they reproduce, as the first stages in the formation of the higher animals, the main and essential features in the life-history of the lowest forms. The "blastoderm" as it is called, or first germ of the embryo in the egg, divides itself into two layers, corresponding, as Huxley has shown, to the two layers into which the body of the Cœlenterata may be divided. Not only so, but most embryos at an early stage of development have the form of a cup, the walls of which are formed by the two layers of the blastoderm. Kowalevsky was the first to show the prevalence of this embryonic form, and subsequently Lankester and Hæckel put forward the hypothesis that it was the embryonic repetition of an ancestral type, from which all the higher forms are descended. The cavity of the cup is supposed to be the stomach of this simple organism, and the opening of the cup the mouth. The inner layer of the wall of the cup constitutes the digestive membrane, and the outer the skin. To this form Hæckel gave the name Gastræa. It is, perhaps, doubtful whether the theory of Lankester and Hæckel can be accepted in precisely the form they propounded it; but it has had an important influence on the progress of embryology. I cannot quit the science of embryology without alluding to the very admirable work on "Comparative Embryology" by our new general secretary, Mr. Balfour, and also the "Elements of Embryology" which he had previously published in conjunction with Dr. M. Foster.

In 1842, Steenstrup published his celebrated work on the "Alternation of Generations," in which he showed that many species are represented by two perfectly distinct types or

broods, differing in form, structure and habits; that in one of them males are entirely wanting, and that the reproduction is effected by fission, or by buds, which, however, are in some cases structurally indistinguishable from eggs. Steenstrup's illustrations were mainly taken from marine or parasitic species, of very great interest, but not generally familiar, excepting to naturalists. It has since been shown that the common Cynips or Gallfly is also a case in point. It had long been known that in some genera belonging to this group, males are entirely wanting, and it has now been shown by Bassett, and more thoroughly by Adler, that some of these species are double-brooded; the two broods having been considered as distinct genera.

Thus an insect known as *Neuroterus lenticularis*, of which females only occur, produces the familiar oak-spangles so common on the under sides of oak leaves, from which emerge, not *Neuroterus lenticularis*, but an insect hitherto considered as a distinct species, belonging even to a different genus, *Spathegaster baccarum.* In Spathegaster both sexes occur; they produce the currant-like galls found on oaks, and from these galls Neuroterus is again developed. So also the King Charles oak-apples produce a species known as *Teras terminalis*, which descends to the ground, and makes small galls on the roots of the oak. From these emerge an insect known as *Biorhiza aptera*, which again gives rise to the common oak-apple.

It might seem that such enquiries as these could hardly have any practical bearing. Yet it is not improbable that they may lead to very important results. For instance, it would appear that the fluke which produces the rot in sheep, passes one phase of its existence in the black slug, and we are not without hopes that the researches, in which our lamented friend Professor Rolleston was engaged at the time of his death, which we all so much deplore, will lead, if not to the extirpation, at any rate to the diminution, of a pest from which our farmers have so grievously suffered. It was in the year 1839 that Schwann and Schleiden demonstrated the intimate relation in which animals and plants stand to each other, by showing the identity of the laws of development of the elementary parts in the two kingdoms of organic nature. Analogies indeed had been previously pointed out, the presence of cellular tissue in certain parts of animals was known, but Caspar F. Wolff's brilliant memoir had been nearly forgotten; and the tendency of microscopical investigation had rather been to encourage the belief that no real similarity existed; that the cellular tissue of animals was essentially different from that of plants. This had arisen chiefly, perhaps, because fully formed tissues were compared, and it was mainly the study of the growth of cells

which led to the demonstration of the general law of development for all organic elementary tissues.

As regards descriptive biology, by far the greater number of species now recorded have been named and described within the last half-century, and it is not too much to say that not a day passes without adding new species to our lists. A comparison, for instance, of the edition of Cuvier's "Regne Animal," published in 1828, as compared with the present state of our knowledge, is most striking.

Dr. Günther has been good enough to make a calculation for me. The numbers, of course, are only approximate, but it appears that while the total number of animals described up to 1831 was not more than 70,000, the number now is at least 320,000.

Lastly, to show how large a field still remains for exploration, I may add that Mr. Waterhouse estimates that the British Museum alone contains not fewer than 12,000 species of insects which have not yet been described, while our collections do not probably contain anything like one-half of those actually in existence. Further than this, the anatomy and habits even of those which have been described offer an inexhaustible field for research, and it is not going too far to say that there is not a single species which would not amply repay the devotion of a lifetime.

One remarkable feature in the modern progress of biological science has been the application of improved methods of observation and experiment; and the employment in physiological research of the exact measurements employed by the experimental physicist. Our microscopes have been greatly improved: achromatic object-glasses were introduced by Lister in 1829; the binocular arrangement by Wenham in 1856; while immersion lenses, first suggested by Amici, and since carried out under the formula of Abbe, are most valuable. The use of chemical reagents in microscopical investigations has proved most instructive, and another very important method of investigation has been the power of obtaining very thin slices by imbedding the object to be examined in paraffin or some other soft substance. In this manner we can now obtain, say, fifty separate sections of the egg of a beetle, or the brain of a bee.

At the close of the last century, Sprengel published a most suggestive work on flowers, in which he pointed out the curious relation existing between these and insects, and showed that the latter carried the pollen from flower to flower. His observations, however, attracted little notice, until Darwin called attention to the subject in 1862. It had long been known that the cowslip and primrose exist under two forms, about equally numerous, and differing from one another in the arrangements

of their stamens and pistils; the one form having the stamens on the summit of the flower and the stigma half-way down; while in the other the relative positions are reversed, the stigma being at the summit of the tube, and the stamens half-way down. This difference had, however, been regarded as a case of mere variability; but Darwin showed it to be a beautiful provision, the result of which is that insects fertilize each flower with pollen brought from a different plant; and he proved that flowers fertilized with pollen from the other form yield more seed than if fertilized with pollen from the same form, even if taken from a different plant.

Attention having been thus directed to the question, an astonishing variety of most beautiful contrivances have been observed and described by many botanists, especially Hooker, Axel, Delpino, Hildebrand, Bennet, Fritz Müller, and above all Herman Müller and Darwin himself. The general result is that to insects, and especially to bees, we owe the beauty of our gardens, the sweetness of our fields. To their beneficent, though unconscious action, flowers owe their scent and color, their honey — nay, in many cases, even their form. Their present shape and varied arrangements, their brilliant colors, their honey, and their sweet scent are all due to the selection exercised by insects.

In these cases, the relation between plants and insects is one of mutual advantage. In many species, however, plants present us with complex arrangements adapted to protect them from insects; such, for instance, are in many cases, the resinous glands which render leaves unpalatable; the thickets of hairs and other precautions which prevent flowers from being robbed of their honey by ants. Again, more than a century ago, our countryman, Ellis, described an American plant, Dionæa, in which the leaves are somewhat concave, with long lateral spines and a joint in the middle; close up with a jerk, like a rat-trap, the moment any unwary insect alights on them. The plant, in fact, actually captures and devours insects. This observation also remained as an isolated fact until within the last few years, when Darwin, Hooker, and others have shown that many other species have curious and very varied contrivances for supplying themselves with animal food.

As regards the progress of botany in other directions, Mr. Thiselton Dyer has been kind enough to assist me in endeavoring to place the principal facts before you. Some of the most fascinating branches of botany — morphology, histology, and physiology scarcely existed before 1833. In the two former branches, the discoveries of von Mohl are preëminent. He first observed cell-division in 1835, and detected the presence of starch in chlorophyll-corpuscles in 1837, while he first

described protoplasm, now so familiar to us, at least by name, in 1846. In the same year Amici discovered the existence of the embryonic vesicle in the embryo sac, which develops into the embryo when fertilized by the entrance of the pollen-tube into the micropyle. The existence of sexual reproduction in the lower plants was doubtful, or at least doubted by some eminent authorities, as recently as 1853, when the actual process of fertilization in the common bladderwrack of our shores was observed by Thuret, while the reproduction of the larger fungi was first worked out by De Bary in 1863.

As regards lichens, Schwendener proposed, in 1869, the startling theory, now, however, accepted by some of the highest authorities, that lichens are not autonomous organisms, but commensal associations of a fungus parasitic on an alga. With reference to the higher Cryptogams it is hardly too much to say that the whole of our exact knowledge of their life-history has been obtained during the last half-century. Thus in the case of ferns the male organs, or antheridia, were first discovered by Nägeli in 1844, and the archegonia, or female organs, by Saminski, in 1848. The early stages in the development of mosses were worked out by Valentine in 1833. Lastly, the principle of Alternation of Generations in plants was discovered by Hofmeister. This eminent naturalist also, in 1851–4, pointed out the homologies of the reproductive processes in mosses, vascular cryptogams, gymnosperms, and angiosperms.

Geographical Botany can hardly be said to have had any scientific status anterior to the publication of the "Origin of Species." The way had been paved, however, by A. de Candolle and the well-known essay of Edward Forbes — "On the Distribution of the Plants and Animals of the British Isles,"— by Sir J. Hooker's introductory essay to the "Flora of New Zealand," and by Hooker and Thomson's introductory essay to the "Flora Indica." One result of these researches has been to give the *coup-de-grâce* to the theory of an Atlantis. Lastly, in a lecture delivered to the Geographical Society in 1878, Thiselton Dyer himself has summed up the present state of the subject, and contributed an important addition to our knowledge of plant-distribution by showing how its main features may be explained by migration in longitude from north to south without recourse being had to a submerged southern continent for explaining the features common to South Africa, Australia and America.

The fact that systematic and geographical botany have claimed a preponderating share of the attention of British phytologists, is no doubt in great measure due to the ever-expanding area of the British Empire, and the rich botanical treasures which

we are continually receiving from India and our numerous colonies. The series of Indian and Colonial Floras, published under the direction of the authorities at Kew, and the "Genera Plantarum" of Bentham and Hooker, are certainly an honor to our country. To similar causes we may trace the rise and rapid progress of economic botany, to which the late Sir W. Hooker so greatly contributed.

In vegetable physiology some of the most striking researches have been on the effect produced by rays of light of different refrangibility. Daubeny, Draper and Sachs have shown that the light of the red end of the spectrum is more effective than that of the blue, so far as the decomposition of carbon dioxide (carbonic acid) is concerned.

Nothing could have appeared less likely than that researches into the theory of spontaneous generation should have led to practical improvements in medical science. Yet such has been the case. Only a few years ago Bacteria seemed mere scientific curiosities. It had long been known that an infusion — say, of hay — would, if exposed to the atmosphere, be found, after a certain time, to teem with living forms. Even those few who still believe that life would be spontaneously generated in such an infusion, will admit that these minute organisms are, if not entirely, yet mainly, derived from germs floating in our atmosphere; and if precautions are taken to exclude such germs, as in the careful experiments especially of Pasteur, Tyndall, and Roberts, every one will grant that in ninety-nine cases out of a hundred no such development of life will take place. In 1836–7 Cagniard de la Tour and Schwann independently showed that fermentation was no mere chemical process, but was due to the presence of a microscopic plant. But, more than this, it has been gradually established that putrefaction is also the work of microscopic organisms.

These facts have led to most important results in Surgery. One reason why compound fractures are so dangerous, is because, the skin being broken, the air obtains access to the wound, bringing with it innumerable germs, which too often set up putrefying action. Lister first made a practical application of these observations. He set himself to find some substance capable of killing the germs without being itself too potent a caustic, and he found that dilute carbolic acid fulfilled these conditions. This discovery has enabled many operations to be performed which would previously have been almost hopeless.

The same idea seems destined to prove as useful in Medicine as in Surgery. There is great reason to suppose that many diseases, especially those of a zymotic character, have their origin in the germs of special organisms. We know that fever runs a

certain definite course. The parasitic organisms are at first few, but gradually multiply at the expense of the patient, and then die out again. Indeed, it seems to be thoroughly established that many diseases are due to the excessive multiplication of microscopic organisms, and we are not without hope that means will be discovered by which, without injury to the patient, these terrible, though minute, enemies may be destroyed, and the disease thus stayed. *Bacillus anthracis*, for instance, is now known to be the cause of splenic fever, which is so fatal to cattle, and is also communicable to man. At Bradford, for instance, it is only too well-known as the woolsorter's disease. If, however, matter containing the Bacillus be treated in a particular manner, and cattle be then inoculated with it, they are found to acquire an immunity from the fever. The interesting researches of Burdon Sanderson, Greenfield, Koch, Pasteur, Toussaint, and others, seem to justify the hope that we may be able to modify these and other germs, and then by appropriate inoculation to protect ourselves against fever and other acute diseases.

The history of Anæsthetics is a most remarkable illustration how long we may be on the very verge of a most important discovery. Ether, which, as we all know, produces perfect insensibility to pain, was discovered as long ago as 1540. The anæsthetic property of nitrous oxide, now so extensively used, was observed in 1800 by Sir H. Davy, who actually experimented on himself, and had one of his teeth painlessly extracted when under its influence. He even suggests that "as nitrous oxide gas seems capable of destroying pain, it could probably be used with advantage in surgical operations." Nay, this property of nitrous oxide was habitually explained and illustrated in the chemical lectures given in hospitals, and yet for fifty years the gas was never used in actual operations. No one did more to promote the use of anæsthetics than Sir James Y. Simpson, who introduced chloroform, a substance which was discovered in 1831, and which for a while almost entirely superseded ether and nitrous oxide, though, with improved methods of administration, the latter are now coming into favor again.

The only other reference to Physiology which time permits me to make, is the great discovery of the reflex action, as it is called, of the nervous centres. Reflex actions had been long ago observed, and it was known that they were more or less independent of volition. But the general opinion was that these movements indicated some feeble power of sensation independently of the brain, and it was not till the year 1832 that the "reflex action" of certain nervous centres was made known to us by Marshall Hall, and almost at the same period by Johannes Müller.

Few branches of science have made more rapid progress in the last half-century than that which deals with the ancient condition of man. When our Association was founded it was generally considered that the human race suddenly appeared on the scene, about 6,000 years ago, after the disappearance of the extinct mammalia, and when Europe, both as regards physical conditions and the other animals by which it was inhabited, was pretty much in the same condition as in the period covered by Greek and Roman history. Since then the persevering researches of Layard, Rawlinson, Botta and others have made known to us, not only the statutes and palaces of the ancient Assyrian monarch, but even their libraries; the cuneiform characters have been deciphered, and we can not only see, but read in the British Museum, the actual contemporary records, on burnt clay cylinders, of the events recorded in the historical books of the Old Testament and in the pages of Herodotus. The researches in Egypt also seem to have satisfactorily established the fact that the pyramids themselves are at least 6,000 years old, while it is obvious that the Assyrian and Egyptian monarchies cannot suddenly have attained to the wealth and power, the state of social organization, and progress in the arts, of which we have before us, preserved by the sand of the desert from the ravages of man, such wonderful proofs.

In Europe, the writings of the earliest historians and poets indicated that, before iron came into general use, there was a time when bronze was the ordinary material of weapons, axes, and other cutting implements, and though it seemed *à priori* improbable that a compound of copper and tin should have preceded the simple metal iron, nevertheless the researches of archæologists have shown that there really was in Europe a "Bronze Age," which at the dawn of history was just giving way to that of "Iron."

The contents of ancient graves, buried in many cases so that their owner might carry some at least of his wealth with him to the world of spirits, have proved very instructive. More especially the results obtained by Nilsson in Scandinavia, by Hoare and Borlase, Bateman and Greenwell, in our own country, and the contents of the rich cemetery at Hallstadt, left no room for doubt as to the existence of a Bronze Age; but we get a completer idea of the condition of Man at this period from the Swiss lake-villages, first made known to us by Keller, and subsequently studied by Morlot, Troyon, Desor, Rütimeyer, Heer, and other Swiss archæologists. Along the shallow edges of the Swiss lakes there flourished, once upon a time, many populous villages or towns, built on platforms supported by piles, exactly as many Malayan villages are now. Under these circumstances innumerable objects were one by one dropped

into the water; sometimes whole villages were burnt, and their contents submerged; and thus we have been able to recover, from the waters of oblivion in which they had rested for more than 2,000 years, not only the arms and tools of this ancient people, the bones of their animals, their pottery and ornaments, but the stuffs they wore, the grain they had stored up for future use, even fruits and cakes of bread.

But this bronze-using people were not the earliest occupants of Europe. The contents of ancient tombs give evidence of a time when metal was unknown. This also was confirmed by the evidence then unexpectedly received from the Swiss lakes. By the side of the bronze-age villages were others, not less extensive, in which, while implements of stone and bone were discovered literally by thousands, not a trace of metal was met with. The shell-mounds or refuse-heaps accumulated by the ancient fishermen along the shores of Denmark, and carefully examined by Steenstrup, Worsaae, and other Danish naturalists, fully confirmed the existence of a "Stone Age."

We have still much to learn, I need hardly say, about this Stone-age people, but it is surprising how much has been made out. Evans truly observes, in his admirable work on "Ancient Stone Implements," "that so far as external appliances are concerned, they are almost as fully represented as would be those of any existing savage nation by the researches of a painstaking traveler." We have their axes, adzes, chisels, borers, scrapers, and various other tools, and we know how they made and how they used them; we have their personal ornaments and implements of war; we have their cooking utensils; we know what they ate and what they wore; lastly, we know their mode of sepulture and funeral customs. They hunted the deer and horse, the bison and urus, the bear and the wolf, but the reindeer had already retreated to the North.

No bones of the reindeer, no fragment of any of the extinct mammalia, have been found in any of the Swiss lake-villages or in any of the thousands of the tumuli which have been opened in our own country or in Central and Southern Europe. Yet the contents of caves and of river-gravels afford abundant evidence that there was a time when the mammoth and rhinoceros, the musk-ox and reindeer, the cave lion and hyena, the great bear and the gigantic Irish elk wandered in our woods and valleys, and the hippopotamus floated in our rivers; when England and France were united, and the Thames and the Rhine had a common estuary. This was long supposed to be before the advent of man. At length, however, the discoveries of Boucher de Perthes in the valley of the Somme, supported as they are by the researches of many continental naturalists, and in our own country of MacEnery and Godwin-Austen, Prestwich

and Lyell, Vivian and Pengelly, Christy, Evans and many more, have proved that man formed a humble part of this strange assembly.

Nay, even at this early period there were at least two distinct races of men in Europe; one of them—as Boyd Dawkins has pointed out—closely resembling the modern Esquimaux in form, in his weapons and implements, probably in his clothing, as well as in so many of the animals with which he was associated.

At this stage Man appears to have been ignorant of pottery, to have had no knowledge of agriculture, no domestic animals, except perhaps the dog. His weapons were the axe, the spear, and the javelin; I do not believe he knew the use of the bow, though he was probably acquainted with the lance. He was, of course, ignorant of metal, and his stone implements, though skilfully formed, were of quite different shapes from those of the second Stone age, and were never ground. This earlier Stone period, when man coëxisted with these extinct mammalia, is known as Palæolithic, or Early Stone Age, in opposition to the Neolithic, or Newer Stone Age.

The remains of the mammalia which coëxisted with man in pre-historic times have been most carefully studied by Owen, Lartet, Rütimeyer, Falconer, Busk, Boyd-Dawkins, and others. The presence of the mammoth, the reindeer, and especially of the musk-ox, indicates a severe, not to say an arctic, climate, the existence of which, moreover, was proved by other considerations; while, on the contrary, the hippopotamus requires considerable warmth. How then is this association to be explained?

While the climate of the globe is, no doubt, much affected by geographical conditions, the cold of the glacial period was, I believe, mainly due to the eccentricity of the earth's orbit combined with the obliquity of the ecliptic. The result of the latter condition is a period of 21,000 years, during one-half of which the northern hemisphere is warmer than the southern, while during the other 10,500 years the reverse is the case. At present we are in the former phase, and there is, we know, a vast accumulation of ice at the south pole. But when the earth's orbit is nearly circular, as it is at present, the difference between the two hemispheres is not very great; on the contrary, as the eccentricity of the orbit increases the contrast between them increases also. This eccentricity is continually oscillating between certain limits, which Croll and subsequently Stone have calculated out for the last million years. At present the eccentricity is ·016 and the mean temperature of the coldest month in London is about 40°. Such has been the state of things for nearly 100,000 years;

but before that there was a period, beginning 300,000 years ago, when the eccentricity of the orbit varied from ·26 to ·57. The result of this would be greatly to increase the effect due to the obliquity of the orbit; at certain periods the climate would be much warmer than at present, while at others the number of days in winter would be twenty more, and of summer twenty less than now, while the mean temperature of the coldest month would be lowered 20°. We thus get something like a date for the last glacial epoch, and we see that it was not simply a period of cold, but rather one of extremes, each beat of the pendulum of temperature lasting for no less than 21,000 years. This explains the fact that, as Morlot showed in 1854, the glacial deposits of Switzerland, and, as we now know, those of Scotland, are not a single uniform layer, but a succession of strata, indicating very different conditions. I agree also with Croll and Geikie in thinking that these considerations explain the apparent anomaly of the coëxistence in the same gravels of arctic and tropical animals; the former having lived in the cold, while the latter flourished in the hot, periods.

It is, I think, now well established that man inhabited Europe during the milder periods of the glacial epoch. Some high authorities, indeed, consider that we have evidence of his presence in pre-glacial and even in Miocene times, but I confess that I am not satisfied on this point. Even the more recent period carries back the record of man's existence to a distance so great as altogether to change our views of ancient history.

Nor is it only as regards the antiquity and material condition of man in prehistoric times that great progress has been made. If time permitted, I should have been glad to have dwelt on the origin and development of language, of custom, and of law. On all of these the comparison of the various lower races still inhabiting so large a portion of the earth's surface, has thrown much light; while even in the most cultivated nations we find survivals, curious fancies, and lingering ideas; the fossil remains, as it were, of former customs and religions embedded in our modern civilization, like the relics of extinct animals in the crust of the earth.

In geology the formation of our Association coincided with the appearance of Lyell's "Principles of Geology," the first volume of which was published in 1830 and the second in 1832. At that time the received opinion was that the phenomena of Geology could only be explained by violent periodical convulsions, and a high intensity of terrestrial energy culminating in repeated catastrophes. Hutton and Playfair had indeed main-

tained that such causes as those now in operation, would, if only time enough were allowed, account for the geological structure of the earth; nevertheless the opposite view generally prevailed, until Lyell, with rare sagacity and great eloquence, with a wealth of illustration and most powerful reasoning, convinced geologists that the forces now in action are powerful enough, if only time be given, to produce results quite as stupendous as those which Science records.

As regards stratigraphical geology, at the time of the first meeting of the British Association at York, the strata between the carboniferous limestone and the chalk had been mainly reduced to order and classified, chiefly through the labors of William Smith. But the classification of all the strata lying above the chalk and below the carboniferous limestone respectively, remained in a state of the greatest confusion. The year 1831 marks the period of the commencement of the joint labors of Sedgwick and Murchison, which resulted in the establishment of the Cambrian, Silurian, and Devonian systems. Our Pre-Cambrian strata have recently been divided by Hicks into four great groups of immense thickness, and implying, therefore, a great lapse of time; but no fossils have yet been discovered in them. Lyell's classification of the Tertiary deposits; the result of the studies which he carried on with the assistance of Deshayes and others, was published in the third volume of the "Principles of Geology" in 1833. The establishment of Lyell's divisions of Eocene, Miocene and Pliocene, was the starting-point of a most important series of investigations by Prestwich and others of these younger deposits; as well as of the Post-tertiary, Quaternary, or drift beds, which are of special interest from the light they have thrown on the early history of man.

A full and admirable account of what has recently been accomplished in this department of science, especially as regards the paleozoic rocks, will be found in Etheridge's late address to the Geological Society.

Before 1831 the only geological maps of this country were William Smith's general and county maps, published between the years 1815 and 1824. In the year 1832 De la Beche made proposals to the Board of Ordnance to color the ordnance-maps geologically, and a sum of 300*l.* was granted for the purpose. Out of this small beginning grew the important work of the Geological Survey.

The cause of slaty cleavage had long been one of the great difficulties of geology. Sedgwick suggested that it was produced by the action of crystalline or polar forces. According to this view miles and miles of country, comprising great mountain masses, were neither more nor less than parts of a gigantic

crystal. Sharpe, however, called attention to the fact that shells and other fossils contained in slate rocks are compressed in a direction at right angles to the planes of cleavage, as if the rocks had been seized in the jaws of a gigantic vise. Sorby first maintained that the cleavage itself was due to pressure. He observed slate rocks containing small plates of mica, and that the effect of pressure would tend to arrange these plates with their flat surfaces perpendicular to the direction of the pressure. Tyndall has since shown that the presence of flat flakes is not necessary. He proved by experiment that pure wax could be made by pressure to split into pieces of great tinuity, which he attributes mainly to the lateral sliding of the particles of the wax over each other; and thus the result of pressure on such a mass is to develop a fissile structure similar to that produced in wax on a small scale, or on a great one in the slate rocks of Cumberland or Wales.

The difficult problem of the conditions under which granite and certain other rocks were formed was attacked by Sorby with great skill in a paper read before the Geological Society in 1858. The microscopic hollows in many minerals contain a liquid which does not entirely fill the hollow, but leaves a small vacuum; and Sorby ingeniously pointed out that the rock must have solidified at least at a temperature high enough to expand the liquid so as to fill the cavity. Sorby's important memoir laid the foundation of microscopic petrography, which is now not only one of the most promising branches of geological research, but which has been successfully applied by Sorby himself, and by Maskelyne, to the study of meteorites.

As regards the physical character of the earth, two theories have been held: one, that of a fluid interior covered by a thin crust; the other, of a practically solid sphere. The former is now very generally admitted, both by astronomers and geologists, to be untenable. The prevailing feeling of geologists on this subject has been well expressed by Professor LeConte, who says, "the whole theory of igneous agencies—which is little less than the whole foundation of theoretic geology—must be reconstructed on the basis of a solid earth."

In 1837 Agassiz startled the scientific world by his "Discours sur l'ancienne extension des Glaciers," in which, developing the observation already made by Charpentier and Venetz, that bowlders had been transported to great distances, and that rocks far away from, or high above, existing glaciers, are polished and scratched by the action of ice, he boldly asserted the existence of a "glacial period," during which Switzerland the North of Europe were subjected to great cold and buried under a vast sheet of ice.

The ancient poets described certain gifted mortals as privi-

leged to descend into the interior of the earth, and have exercised their imagination in recounting the wonders there revealed. As in other cases, however, the realities of science have proved more varied and surprising than the dreams of fiction. Of the gigantic and extraordinary animals thus revealed to us by far the greatest number have been described during the period now under review. For instance, the gigantic Cetiosaurus was described by Owen in 1838, the Dinornis of New Zealand by the same distinguished naturalist in 1839, the Mylodon in the same year, and the Archæopteryx in 1862.

In America, a large number of remarkable forms have been described, mainly by Marsh, Leidy and Cope. Marsh has made known to us the Titanosaurus, of the American (Colorado) Jurassic beds, which is, perhaps, the largest land animal yet known, being a hundred feet in length, and at least thirty in height, though it seems possible that even these vast dimensions were exceeded by those of the Atlantosaurus. Nor must I omit the Hesperornis, described by Marsh in 1872, as a carnivorous, swimming ostrich, provided with teeth, which he regards as a character inherited from reptilian ancestors; the Ichthyornis, stranger still, with biconcave vertebræ, like those of fishes, and teeth set in sockets; while in the Eocene deposits in the Rocky Mountains the same indefatigable paleontologist, among other very interesting remains, has discovered three new groups of remarkable mammals, the Dinocerata, Tillodontia, and Brontotheridæ. He has also described a number of small, but very interesting, Jurassic mammalia, closely related to those found in our Stonesfield Slate and Purbeck beds, for which he has proposed a new order, "Prototheria." Lastly, I may mention the curiously anomalous Reptilia from South Africa, which have been made known to us by Professor Owen.

Another important result of recent paleontological research is the law of brain-growth. It is not only in the higher mammalia that we find forms with brains much larger than any existing, say, in Miocene times. The rule is almost general that — as Marsh has briefly stated it — "all tertiary animals had small brains." We may even carry the generalization further. The Cretaceous birds had brains one-third smaller than those of our own day, and the brain-cavities of the Dinosauria of the Jurassic period, are much smaller than in any existing reptiles.

As giving, in a few words, an idea of the rapid progress in this department, I may mention that Morris's "Catalogue of British Fossils," published in 1843, contained 5,300 species; while that now in preparation by Mr. Etheridge enumerates 15,000.

But if these figures show how rapid our recent progress has been, they also very forcibly illustrate the imperfection of the geological record, and give us, I will not say a measure, but an idea, of the imperfection of the geological record. The number of all the described recent species is over 300,000, but certainly not half are yet on our lists, and we may safely take the total number of recent species as being not less than 700,000. But in former times there have been at the very least twelve periods, in each of which by far the greater number of species were distinct. True the number of species was probably not so large in the earlier periods as at present; but if we make a liberal allowance for this, we shall have a total of more than 2,000,000 species, of which about 25,000 only are as yet upon record; and many of these are only represented by a few, some only by a single specimen, or even only by a fragment.

The progress of paleontology may also be marked by the extent to which the existence of groups has been, if I may so say, carried back in time. Thus I believe that in 1830, the earliest known quadrupeds were small marsupials belonging to the Stonesfield slates; the most ancient mammal now known is Microlestes antiquus from the Keuper of Würtemberg; the oldest bird known in 1831 belonged to the period of the London Clay, the oldest now known is the Archæopteryx of the Solenhofen slates, though it is probable that some at any rate of the footsteps on the Triassic rocks are those of birds. So again the Amphibia have been carried back from the Trias to Coal-measures; Fish from the Old Red Sandstone to the Upper Silurian; Reptiles to the Trias; Insects from the Cretaceous to the Devonian; Mollusca and Crustacea from the Silurian to the Lower Cambrian. The rocks below the Cambrian, though of immense thickness, have afforded no relics of animal life, if we except the problematical Eozoon Canadense, so ably studied by Dawson and Carpenter. But if paleontology as yet throws no light on the original forms of life, we must remember that the simplest and the lowest organisms are so soft and perishable that they would leave "not a wrack behind." I will not, however, enlarge on this branch of science, because we shall have the advantage on Friday of hearing it treated with the skill of a master.

Passing the Science of Geography, Mr. Clements Markham has recently published an excellent summary of what has been accomplished during the half-century.

As regards the Arctic regions, in the year 1830 the coast line of Arctic America was only very partially known, the region between Barrow Strait and the continent, for instance,

being quite unexplored, while the eastern sides of Greenland and Spitzbergen, and the coasts of Nova Zembla were almost unknown. Now the whole coast of Arctic America has been delineated, the remarkable archipelago to the north has been explored, and no less than seven northwest passages — none of them, however, of any practical value — have been traced. The northeastern passage, on the other hand, so far at least as the mouths of the great Siberian rivers, may perhaps hereafter prove of commercial importance. In the Antarctic regions, Enderby and Graham Lands were discovered in 1831–2, Balleny Islands and Sabrina Land in 1839, while the fact of the existence of the great southern continent was established in 1841 by Sir James Ross, who penetrated in 1842 to 78° 11′, the southernmost point ever reached.

In Asia, to quote from Mr. Markham, "our officers have mapped the whole of Persia and Afghanistan, surveyed Mesopotamia, and explored the Pamir steppe. Japan, Borneo, Siam, the Malay peninsula, and the greater part of China have been brought more completely to our knowledge. Eastern Turkestan has been visited, and trained native explorers have penetrated to the remotest fountains of the Oxus, and the wild plateaus of Tibet. Over the northern half of the Asiatic Continent the Russians have displayed great activity. They have traversed the wild steppes and deserts of what on old atlases was called Independent Tartary, have surveyed the courses of the Jaxartes, the Oxus and the Amur, and have navigated the Caspian and the Sea of Aral. They have pushed their scientific investigations into the Pamir and Eastern Turkestan, until at last the British and Russian surveys have been connected."

Again, fifty years ago the vast Central Regions of Africa were almost a blank upon our best maps. The rudely drawn lakes and rivers in maps of a more ancient date had become discredited. They did not agree among themselves, the evidence upon which they were laid down could not be found, they were in many respects highly improbable, and they seemed inconsistent with what had then been ascertained concerning the Niger and the Blue and White Niles. At the date of which I speak, the Sahara had been crossed by English travelers from the shore of the Mediterranean: but the southern desert still formed a bar to travelers from the Cape, while the accounts of traders and others who alone had entered the country from the eastern and western coasts were considered to form an insufficient basis for a map.

Since that time the successful crossing of the Kalahari desert to Lake Ngami has been the prelude to an era of African discovery. Livingston explored the basin of the Zambesi, and

discovered vast lakes and waters which have proved to be those of the higher Congo. Burton and Speke opened the way from the West Coast, which Speke and Grant pursued into and down the Nile, and Stanley down the course of the middle and lower Congo; and the vast extension of Egyptian dominion has brought a huge slice of equatorial Africa within the limits of semi-civilization. The western side of Africa has been attacked at many points. Alexander and Galton were among the first to make known to us its western tropical regions immediately to the north of the Cape Colony; the Ogowé has been explored; the Congo promises to become a center of trade, and the navigable portions of the Niger, the Gambia, and the Senegal are familiarly known.

The progress of discovery in Australia has been as remarkable as that in Africa. The interior of this great continent was absolutely unknown to us fifty years ago, but is now crossed through its center by the electric telegraph, and no inconsiderable portion of it is turned into sheep-farms. It is an interesting fact that General Sabine, so long one of our most active officers, and who is still with us, though, unfortunately, his health has for some time prevented him from attending our meetings, was born on the very day that the first settler landed in Australia.

[To be continued.]

Art. XL.—*Notes on Earthquakes*; by C. G. Rockwood.

The Scio Earthquake.—In April last the Island of Scio and its vicinity were shaken by an earthquake which caused great loss of life and property and proved to be the beginning of quite an extended series of shocks. This Island lies off the Gulf of Smyrna in the Grecian Archipelago and is about thirty-two miles long north and south, by about eighteen miles wide. It is separated from the mainland by a strait seven or eight miles wide and had about 50,000 inhabitants. The first and most violent shock occurred at 1.40 P. M. on Sunday, April 3d, and lasted ten seconds. It was followed by a second at 2 P. M. and a third at 3 P. M. of the same day. The ground was then quiet until sunset, when the shaking recommenced and continued with such frequency that up to April 5th two hundred and fifty shocks had been counted; of which thirty or forty were of sufficient strength to overthrow walls. Other shocks, often severe, occurred from time to time up to May 20th. An especially severe one, lasting four seconds, occurred on April 11th. The violent shocks with which the disturbance

began, destroyed many villages, and especially damaged the city of Scio or Kastro, on the east coast, the chief town of the island. It was estimated that in all the southern part of the island certainly nine-tenths of the houses would have to be rebuilt and some whole villages were reduced to simple masses of ruins. The loss of life was at first estimated as high as 10,000, but later advices render it probable that not more than 3000 or 4000 were killed. The consequent suffering and destitution were, however, so great that contributions were made in various countries of Europe and America for the relief of the survivors. The center of disturbance appears to have been under the eastern part of the island and the vibrations were felt with destructive effect at Tchisme and at Smyrna on the mainland to the eastward, and Eubœa and the islands of Tinos and Syra to the westward. The direction of vibration was east and west, as is shown not only by numerous personal reports, but by the direction of the cracks in the broken walls.

Die Vulkanischen Ereignisse des Jahres 1880.—The *Sixteenth Annual Report* of Dr. C. W. C. FUCHS (Mineralog. u. Petrograph. Mittheil., Wien.) is at hand and presents some points of interest.

The volcanic activity of the year was less than usual, no great eruption having occurred anywhere. From Vesuvius small streams of lava issued in February and toward the end of July, and again in September, October and November. So also Etna showed some activity in February which lasted until May, consisting, however, mostly of showers of ashes. Other eruptive phenomena were the sand shower in St. Domingo on January 4, the elevation of the Island in Lake Ilopango in January, the eruption of Fuego in Guatemala June 29, and the eruption of Mauna Loa Nov. 5.

Earthquakes are recorded to the number of 225, of which 65 are American, showing that the deficiency of such items in previous reports was due, as was supposed, to want of full information, and not to any dearth of such phenomena upon the Western continent. Of these 65, all but one have already been noted in this Journal. The earthquakes of the year were divided among the seasons as follows:

Winter, 80—Dec. 43, Jan. 18, Feb. 19;
Spring, 32—March 15, April 9, May, 8;
Summer, 59—June 10, July 28, August 21;
Autumn, 54—Sept. 14, Oct. 9, Nov. 31.

On thirty-three days in the year shocks occurred at two or more distant places, and thirty-two places were affected at two or more times. A few earthquakes are of sufficient interest to merit more special notice.

Those of San Salvador in January and February, in Cuba, Florida and Mexico in the latter part of January, and the destructive shocks in the Philippines in July, have already been mentioned in this Journal.

On November 9, the city of Agram, after numerous less important shakings during the summer, was affected by a violent earthquake, which extended over Croatia, Montenegro, and a great part of Hungary and Bosnia, and even to Bohemia and upper Italy. This, the most severe shock, was followed by numerous others in gradually decreasing intensity, so that up to the 18th December, 61 distinct shocks had been observed in the city, with minor vibrations innumerable. The city appears to have suffered frequently in the past, as a list is given of 33 earthquakes which have occurred there since 1502. The author remarks on the continuance of the subterranean noises when the shocks had ceased and the ground was at rest. The phenomena still continued at the end of the year.

On July 4 all Switzerland was shaken by an earthquake, which had its origin in the neighborhood of the Simplon.

Smyrna and its vicinity suffered on the 22d of June, and again on the 29th of July, when the shocks extended to the neighboring islands and did much damage. In Smyrna itself one hundred houses were overthrown and thirty persons were killed. The centre of disturbance was in the mountains northeast of the city, where the village of Menemen was left uninhabitable. This earthquake has been described in the French scientific journals. This same region has again been shaken by the Scio earthquake of 1881, as mentioned above.

On September 2d an earthquake at Kalavrita, in the Peloponnesus, was felt also on the other side of the Mediterranean at Tripoli, in Africa.

Dr. Fuchs records some observations on the slight vibrations which Prof. Perrey has reported as occurring frequently in Nice. They are only perceptible at night when all is still, and he is inclined to refer them to the dashing of the waves upon the shore, although he states that the intensity of the vibration does not correspond to that of the wave action, nor yet do the intervals between the vibrations correspond to the intervals between the waves. He suggests that the *direction* in which the waves strike may have influence on the phenomena.

C. G. R.

ART. XLI.—*Notice of the remarkable Marine Fauna occupying the outer banks off the Southern coast of New England, No. 2*; by A. E. VERRILL. (Brief Contributions to Zoology from the Museum of Yale College: No. XLVIII.)

THE U. S. Fish Commission has occupied, this season, the station at Wood's Holl,* Mass., on Vineyard Sound, where a laboratory for its use was established in 1875.

The shallower waters of that region had been very fully explored by the Fish Commission in 1871 and 1875. Nevertheless, much has been done this year toward completing the investigation of the surface fauna, which is exceedingly rich and varied at Wood's Holl. The larval forms of crustacea, annelida, echinodermata, mollusca, etc., have been taken in large numbers in the towing nets, as well as adult forms of many kinds, including, especially, numerous species of Syllidæ, many of which are new.

The special subject for investigation this year, was, however, the rich fauna that was last year discovered in deep water, about 75 to 120 miles off the southern coast of New England, near the edge of the Gulf Stream. A brief account of our discoveries in that region last season was published by me in this Journal (vol. xx, p. 390), with notices and descriptions of many of the mollusca and echinoderms then discovered. A more detailed account of the mollusca was published by me in the Proceedings of the National Museum (vol. iii, pp. 356–409, Dec.–Jan.). Professor S. I. Smith published an account of the crustacea in the same Proceedings (vol. iii, pp. 413-452, Jan., 1881).

In the following articles I propose to notice some of the more interesting species, whether obtained this year or last year. Some of these species were also dredged on the 16th of last November, by Lieut. Z. L. Tanner, in a trip made to the deep water off the mouth of Chesapeake Bay, after the regular dredging operations of the season had ceased.

As many of the species there obtained are referred to, a list of the stations is here added:

Station.	Locality. N. Lat.	W. Long.	Fathoms.	Bottom.
896	37° 26′	74° 19′	56	sand, shells.
897	37 25	74 18	157½	sand, mud.
898	37 24	74 17	300	mud.
899	37 22	74 29	57½	sand.
900	37 19	74 41	31	sand.
901	37 10	75 08	18	sand.

Our dredgings this year, in deep water, have also been made with the "Fish Hawk," Lieut. Z. L. Tanner, commander. Mr.

* Formerly written "Wood's Hole," but the name was changed by an act of the Legislature of Massachusetts, in 1875.

A. P. Chapin, of Warsaw, N. Y., made the temperature observations and records of soundings, etc.

The party immediately associated with the writer in the zoological investigations consisted of Professor S. I. Smith and Mr. J. H. Emerton (artist), of Yale College; Dr. T. H. Bean and Mr. Richard Rathbun, of the National Museum; Mr. Sanderson Smith, of New York; Professor L. A. Lee, of Bowdoin College; Mr. B. F. Koons, Mr. E. A. Andrews, and Mr. H. L. Bruner, graduates and special zoological students of the Sheffield Scientific School of New Haven, and Mr. Peter Parker, of Washington. Most of these gentlemen have been associated with me, in the same way, in previous years.

The off-shore regions explored this year are included between N. lat. 39° 40′ and 40° 22′; and between W. long. 69° 15′ and 71° 32′. They occupy a region about 42 miles wide, north and south; and about 95 miles long, east and west, or about 105 miles along the 100-fathom line.

Series of dredgings have also been made this season, off Cape Cod; in Vineyard Sound; in Buzzard's Bay; and off Martha's Vineyard, between the deep-water and shallow-water localities of former years. Other dredgings will be made later, this season.

It is probable that the remarkable richness of the fauna in this region, both in the number of species and in the surprising abundance of the individuals of many of them, is due very largely to the unusual uniformity of the temperature enjoyed, at all seasons of the year, at all those depths that are below the immediate effects of the atmospheric changes. The region under discussion is subject to the combined effects of the Gulf Stream on one side and the cold northern current on the other, together with the gradual decrease in temperature in proportion to the depth. It is, therefore, probable that at any given depth, below 50 fathoms, the temperature is nearly the same at all seasons of the year. Moreover, there is, in this region, an active circulation of the water, at all times, due to the combined currents and tides. The successive zones of depth represent successively cooler climates more perfectly here than near the coast. The vast quantities of free-swimming animals, continually brought northward by the Gulf Stream, and filling the water, both at the surface and bottom, furnish an inexhaustible supply of food for many of the animals inhabiting the bottom, and probably, directly or indirectly, to nearly all of them. A very large species of *Salpa*, often five or six inches long, occurs both at the surface and close to the bottom, in vast quantities. Sometimes several bushels come up in a single haul of the trawl. I have taken this same *Salpa*, in very numerous instances, from the stomachs of starfishes of many kinds, from Actiniæ of

Table of Outer Stations, occupied in 1881, with Temperatures of bottom and surface.

The distances are measured from Gay Head Light, in geographical miles. The bearings are magnetic.

Stat.	Locality.	Fath.	Bottom.	Date.	Temperature. Bott'm.	Temperature. Surf'ce	Hour.
	Off Martha's Vineyard.						
917	S. ½ W. 59½ m.	43	gn. mud	July 16	42° F.	63° F.	4.10 A. M.
918	" 61 "	45	"	"	45	63	5.33 "
919	" 65 "	51½	"	"	42·5	66	7.00 "
920	" 68½ "	61	"	"	49	66	8.20 "
921	" 73 "	65	"	"	52	70	9.40 "
922	" 77 "	69	gn. m. sd.	"	52	72	10.57 "
923	" 78½ "	96	sand	"	52	72	12.37 P. M.
924	" 83½ "	160	"	"	44·5	71	1.52 "
925	" 86 "	224	sd. m.	"	42	71	3.35 "
926	" 85 "	195	"	"	44	71	5.24 "
935	S. by E. ½ E. 106½ m.	770		Aug. 4	39·5	70	8.14 A. M.
936	" " 104½ "	705	mud	"	39·5	71	10.43 "
937	" " 102 "	506	gn. s. m.	"	40·5	72	12.45 P. M.
938	" " 100 "	310	"	"	42	72·5	2.44 "
939	" " 98 "	258	"	"	47	73	4.25 "
940	" " 97 "	130	sand	"	52	72	5.30 "
941	" " 89½ "	76	sd. mud	"	52	71	7.45 "
942	S. by W. ¾ W 81½ "	134	"	" 9	50	69	6.15 A. M.
943	S.S.W. 83 "	153	sd. m. sh.	"	49	70	7.10 "
944	" 82 "	124	"	"	51	70	8.27 "
945	S. by W. ¾ W. 84½ "	202	gn. m. sd.	"	44	71	12.05 P. M.
946	" " 87½ "	241	"	"	47	71	2.00 "
947	" " 89 "	312	sd. m.	"	44	70	4.00 "
949	S. 79½ "	100	y. mud	" 23	52	66	4.20 A. M.
950	" 75 "	69	s. sh. mud	"	52	65	5.50 "
951	" 85 "	219	mud	"	41	67·5	9.40 "
952	S. ¼ E. 87½ "	388	y. m. sd.	"	40	68	11.28 "
953	S. ½ E. 91½ "	716	mud	"	39·5	68	2.30 P. M.
954	" 91 "	642	sd. mud	"	39·5	68	4.50 "
994	S.S.W ½ W. 104½ "	368	mud	Sept. 8	40·5	68	4.50 A. M.
995	" " 104¼ "	358	y. m. sd	"	40·5	68	6.32 "
996	" " 104 "	346	"	"	40	67·5	7.35 "
997	" " 103½ "	335	y. m.	"	40	67·5	9.03 "
998	" " 102½ "	302	gr. m.	"	40	68	10.34 "
999	" " 100 "	266	"	"		68	11.48 "
1025	S.S.W. ½ W. 95 "	216	"	"	45	69	1.05 P. M.
1026	" " 93½ "	182	"	"	47·5	69	2.55 "
1027	S.S.E. ¾ E. 105½ "	93	fine sand.	" 14	48½	65	7.23 A. M.
1028	" " 108½ "	410	y. mud.	"	41	66	9.01 "
1029	" " 109½ "	458	"	"	40	68	12.13 P. M.
1030	" ⅞ 108½ "	337	"	"	41	66	1.52 "
1031	" ⅝ 107½ "	255	"	"	46	65	2.54 "
1032	" ½ 107 "	208	"	"	46	65	4.00 "
1033	" " 106 "	183	sd. gravel.	"		63	4.55 "
1034	" " 105½ "	146	sd. y. mud.	"	46½	62	5.55 "
1035	" " 103½ "	120	sand.	"	47	62	6.56 "
1036	" " 102 "	94	sand.	"	51	61½	7.54 "

several species, etc. Pteropods also frequently occur in the stomachs of the starfishes, while Foraminifera furnish a large part of the food of many of the mud-dwelling species.

The fishes, which are very abundant and of many species, find a wonderfully abundant supply of most excellent food in the very numerous species of crabs, shrimp and other Crustacea, which occur in such vast quantities, that not unfrequently many thousands of specimens of several species are taken in a single haul of the trawl. Cephalopods are also abundant and are eagerly devoured by the larger fishes, while others prey largely upon the numerous gastropods and bivalves.

Fishes.

The fishes obtained by us are of great interest. The large number of species taken will be indicated by the accompanying list, which has been kindly made out for me by Dr. T. H. Bean, who has had charge of the fishes this season. A considerable number of species, not included in this list, are either undescribed or not fully identified. These will soon be published in a more detailed list.

The new species of fishes taken in 1880, in this region, were described by Mr. G. Brown Goode, and a list of the 51 species, obtained by us, was also published by him (Proc. Nat. Mus., iii, pp. 337–467, Nov., 1880, and Feb., 1881).

The most important of the fishes, is the *Lopholatilus chamæleonticeps* Goode and Bean, or "Tile-fish." This is a large and handsome edible fish, first discovered on these grounds in 1879, and not yet found elsewhere. It seems to be very abundant over the whole region explored by us, in 70 to 134 fathoms. On one occasion a "long-line" or "trawl-line" was put down at station 949, in 100 fathoms, and 73 of these fishes were taken, weighing 541 pounds. These varied in weight from 2½ to 31 pounds. It is brownish gray, more or less covered with large bright yellow spots. The *Peristedium miniatum* Goode, is a very curious and handsomely colored fish, often bright red throughout. The several species of "hake" (*Phycis*) are common, as well as the "whiting" (*Merlucius bilinearis*). Large specimens of the "goose-fish" or "angler" are often taken in the trawl, in as much as 250 fathoms.

List of Fishes. By Dr. T. H. Bean.

1. *Halieutæa senticosa* Goode.

Taken at 9 stations; 160 to 335 fathoms; abundant at stations 925 and 951.

2. *Lophius piscatorius* Linn. (Goose-fish).

Stations 919, 944 and 997; 51½ to 335 fathoms; one at each station.

3. *Centriscus scolopax* Linn. (Trumpet-fish.)

One trawled at station 940, in 130 fathoms.

4. *Hippoglossoides platessoides* (Fabr.) Gill. (Flounder.)
Taken sparingly at stations 917 and 918; 43 to 45 fathoms.

5. *Paralichthys oblongus* (Mitch.) Jordan. (4-spotted Flounder.)
Abundant at station 923, in 96 fathoms; a few taken at 917, 919 and 940, in 43 to 130 fathoms.

6. *Monolene sessilicauda* Goode.
A single one caught at 923, in 96 fathoms, sand.

7. *Citharichthys arctifrons* Goode.
Abundant at numerous stations, in 51¼ to 130 fathoms.

8. *Glyptocephalus cynoglossus* (Linn.) Gill. (Pale-flounder.)
Occurred at 13 stations, in 160 to 506 fathoms; abundant at 994.

9. *Macrurus Bairdii* Goode and Bean. (Baird's Grenadier.)
Obtained at 15 stations, in 160 to 506 fathoms; usually abundant.

10. *Macrurus carminatus* Goode. Grenadier.
Taken at 7 stations, in 182 to 396 fathoms; abundant only at 951 and 952.

11. *Phycis chuss* (Walb.) Gill. (Hake.)
Trawled at 5 stations in 43 to 130 fathoms; abundant at 918, 919 and 923.

12. *Phycis tenuis* (Mitch.) DeKay. (Hake.)
Secured at 12 stations, in 43 to 506 fathoms; abundant only at 942.

13. *Phycis Chesteri* Goode and Bean. (Chester's Hake.)
Caught at 15 stations, in 160 to 506 fathoms; generally abundant.

14. *Physiculus Dalwigkii* Kaup.?
A single young individual was taken at station 952, in 396 fathoms.

15. *Physiculus*, sp.
Three young examples were obtained at station 941, in 76 fathoms.

16. *Enchelyopus cimbrius* (Linn.) Jordan. (Rockling.)
Taken in small numbers at 918, 946, 951 and 998; 45 to 302 fathoms.

17. *Merlucius bilinearis* (Mitch.) Gill. (Whiting.)
Found at 13 stations, in 100 to 312 fathoms; usually scarce at these depths.

18. *Ophidium*, sp. undetermined.
14 individuals were trawled at station 941, in 76 fathoms.

19. *Lycodes Vahlii* Reinhardt.
A single individual at each of two stations, 952 and 998, 302 to 396 fathoms.

20. *Lycodes Verrillii* Goode and Bean.
Taken at 11 stations, in 216 to 368 fathoms; never abundant.

21. *Zoarces anguillaris* (Peck) Storer. (Eel-pout.)
One obtained at station 918, in 45 fathoms.

22. ? *Liparis vulgaris* Fleming.
Found at station 918, in 45 fathoms, in the gill-cavity of *Pecten tenuicostatus*.

23. *Careproctus Reinhardtii* Kröyer.
Taken in small numbers at 5 stations, in 202 to 310 fathoms.

24. *Peristedium miniatum* Goode.
Rare at stations 922, 940 and 950; 69 to 130 fathoms.

25. *Amitra liparina* Goode.
Found at 5 stations, in 310 to 506 fathoms; not common.

26. *Cottunculus microps* Collett.
Obtained at 7 stations, in 224 to 396 fathoms; not common.

27. *Cottunculus torvus* Goode, MSS.
A single specimen was taken at station 994, in 368 fathoms.

28. *Cottus octodecimspinosus* Mitchill. (Sculpin.)
One individual was trawled at 917, in 43 fathoms.

29. *Sebastes marinus* (Linn.) Lütken. (Rose Fish.)
The only one seen was obtained in 241 fathoms, at station 946.

30. *Setarches parmatus* Goode.
Found at stations 939, 940, 950, in 69 to 258 fathoms; abundant at 940 only.

31. *Lopholatilus chamæleonticeps* Goode and Bean. (Tile Fish.)
8 individuals were caught on a trawl-line, near station 942, in 134 fathoms; and 73 at station 949, in 100 fathoms.

32. *Hoplostethus mediterraneus* Cuv. and Val.
A young example at station 998; one at 1025 and two at 1026; 182 to 302 fathoms.

33. *Scopelus*, species undetermined.
Abundant at several stations, in 182 to 724 fathoms.

34. *Scopelus*, species undetermined.
Found sparingly at several stations, in 182 to 506 fathoms.

35. *Stomias ferox* Reinhardt.
Single individuals were caught at 936, 953 and 995; 358 to 724 fathoms.

36. *Conger vulgaris* Cuv. (Conger Eel.)
One specimen was obtained at 919, and one at 941; 51½ to 76 fathoms.

37. *Nemichthys scolopaceus* Richardson. (Snipe Eel.)
7 individuals were taken at 5 stations, in 216 to 506 fathoms.

38. *Synaphobranchus pinnatus* (Gronow) Günther. (Long-nosed Eel.)
Found at 10 stations, in 219 to 506 fathoms; common.

39. *Simenchelys parasiticus* Gill. (Pug-nosed Eel.)
A few specimens were obtained at station 937, in 506 fathoms.

40. *Raia eglanteria* Lac. (Skate.)
Taken sparingly at 924 and 940, in 130 to 160 fathoms.

41. *Raia lævis* Mitchill. (Barn-door Skate.)
Found at several stations, in 43 to 202 fathoms; abundant at 942 and 949.

42. *Raia radiata* Donovan. (Skate.)
Found in small numbers at stations 924, 946 and 951; 160 to 241 fathoms.

43. *Centroscyllium Fabricii* (Reinh.) Müll. and Henle. (Black Dog-fish.)
Taken at stations 952 and 994, in 368 to 396 fathoms; rare.

44. *Petromyzon marinus* Linn. (Lamprey.)
A single specimen was taken in 241 fathoms, at station 946.

45. *Myxine glutinosa* Linné. (Hag Fish.)
Trawled at 4 stations, in 160 to 258 fathoms; usually rare, but abundant at 951.

MOLLUSCA.

Most of the mollusca recorded in my papers of last year were again obtained this season, and often in larger numbers. A complete list will be published in a future paper. At the present time I shall refer only to some of the more important ones, and to some of those that are additions to the fauna.

Of the Cephalopods, the following species were taken:

Gonatus Fabricii Steenstrup.*

Station 953; 715 fathoms; one large and perfect male specimen. Station 1031; 255 fathoms; one young specimen.

The former is the form recently figured by Steenstrup, under the above name, and considered by him the adult of *Gonatus amœnus*.

* A direct comparison of this individual with the mutilated specimen described, by me, last year, as *Cheloteuthis rapax*, shows that they are probably identical. The latter was separated, as a genus, from *Gonatus*, as understood by Steenstrup (=*Lestoteuthis* Verrill) mainly because the ventral arms appeared to have had two interior rows of *hooks*, like those on the other arms, while in *Gonatus* they are

Ommastrephes illecebrosus Verrill.

Stations 918, 919, 923–925, 939, 940, 949, 1025, 1033; 45–258 fathoms.

Taonius pavo (Les.) Steenstrup.

Station 952; 388 fathoms. Two specimens. This rare species has not been recorded from our coast, since it was described by Lesueur, in 1821.

Rossia sublevis Verrill.

Stations 924, 925, 939, 945–947, 951, 952, 997, 1025, 1026, 1028, 1029, 1032, 1033; 106–388 fathoms. Some of the specimens, recently obtained, agree more nearly with *R. glaucopis* Lov., as figured by G. O. Sars, than any seen before. It may prove to be identical.

Heteroteuthis tenera Verrill.

Stations 918, 919, 920, 921, 922, 940, 944, 949, 950, 1026, 1027; 45–182 fathoms. Eggs of this species were taken at stations 922, 940, 949, and in several localities in 1880. They are nearly round, ivory-white or pearly, attached to shells, etc., by one side, in groups, or scattered. On the upper side there is a small conical eminence.

Sepiola leucoptera Verrill.

Stations 947, 952, 998, 999, 1026 (3 juv.); 182–388 fathoms.

Octopus Bairdii Verrill.

Stations 925, 939, 945–947, 951, 952, 994, 997, 998, 1025, 1026, 1028, 1033, 1035; 103–388 fathoms.

composed of suckers, like the outer rows; but the horny parts had been destroyed, in my specimen, and the hook-shaped form of the fleshy part of the suckers was probably due to post-mortem changes. By careful treatment with reagents I have been able to restore some of the distal ones more completely, so as to show a distinctly sucker-like form.

It would, however, be difficult, without farther evidence, to believe that *Gonatus amœnus*, as figured by G. O. Sars, is the young of this species, for he neither mentions nor figures the remarkable series of lateral connective suckers and tubercles on the tentacular clubs, though he gives detailed figures of the club and its other hooks and suckers. That so careful an observer as Sars should have overlooked such a structure seems almost incredible. The two small specimens that I have hitherto seen from America, agreed well with Sars' figures, but both were considerably injured from having been in fish-stomachs. A small specimen (mantle 30^{mm} long) recently taken by us, at station 1031, is, however, well preserved, and while agreeing with *G. amœnus* in all other respects, it has the peculiar lateral connective suckers and tubercles of the club, seen in *G. Fabricii*, adult. These organs are, however, very minute in this specimen, but sufficiently evident to convince me that Steenstrup is correct in considering *G. amœnus* the young of *G. Fabricii*.

Since Steenstrup has shown that the type of my genus *Lestoteuthis* is the same genus as *Gonatus* (adult), and therefore that *L. robustus* (Dall), doubtfully referred to it by me, is a distinct genus, I propose to make the latter the type of a new genus: *Moroteuthis*. Its most prominent distinctive character will be the remarkable solid cartilaginous cone, superadded to the end of the pen, and corresponding in form and position with the solid cone of *Belemnites*.

Alloposus mollis Verrill.

Stations 937, 938, 952, 953, 994; 310–715 fathoms. Two very large females were taken: one at station 937, in 506 fathoms; the other at 994, in 368 fathoms. The former weighed over 20 pounds. Length from end of body to tip of 1st pair of arms, 31 inches; of 2d pair, 32; of 3d pair, 28; of 4th pair, 28; length of mantle beneath, 7; beak to end of 4th pair of arms, 22; breadth of body, 8·5; breadth of head, 11; diameter of eye, 2·5; of largest suckers, ·38.

The only additional Pteropod taken this year is *Triptera columnella* (Rang), from station 947. Among the Gastropods there are a considerable number of species not obtained last year. Perhaps the most remarkable discovery, in this group, is a fine typical species of *Dolium* (*D. Bairdii*) taken alive, in 202 fathoms. This genus is almost exclusively tropical in its distribution. On our coast, *D. galea* extends northward to North Carolina. This southern form, with a large *Marginella*, taken both this year (station 949) and last, an *Avicula*, and various other genera, more commonly found in southern waters, are curiously associated, in this region, with genera and species which have hitherto been regarded as exclusively northern or even arctic, many of them having been first discovered in the waters of Greenland, Spitzbergen, northern Norway, Jan Mayen Land, etc.

Among the northern species which had not been found previously south of Cape Cod, the following were dredged: *Trophon clathratus*, 972, 976; *Acirsa costulata* (=*borealis*), 965; *Amauropsis Islandica* (=*helicoides*); *Margarita cinerea*, 981; *Machæroplax bella*, 1032; *Cylichna Gouldii*, 973; *Odostomia* (*Menestho*) *striatula*, 980.

Dolium Bairdii Verrill and Smith, sp. nov.

A moderately large species, having nearly the form of *D. perdix* and *D. zonatum*. Male. Shell broad ovate, with seven broadly rounded whorls; spire elevated, apex acute; nuclear whorls about three, smooth; suture impressed, but not deep, nor channelled, the last whorl is somewhat flattened (perhaps abnormally) below the suture, for some distance, corresponding to an inward flexure of the outer lip. Aperture elongated, irregularly ovate; outer lip regularly rounded, except for a short distance posteriorly, where it is slightly incurved, its edge is excurved, acute externally, distinctly but not prominently crenulated within, except posteriorly, where a posterior canal is slightly indicated; columella straight; canal short and broad. The sculpture is peculiar: it consists of numerous (about 40 on the last whorl) rather prominent, squarish, clearly defined revolving ribs, less than 1^{mm} broad, separated by interspaces of about

the same breadth, in which there is usually one small narrow rib, alternating with the larger ones; sometimes there are two or more small ones. The whole surface, both of ribs and interspaces, is covered with fine and regular transverse, raised lines. The surface is covered with a very thin pale olive-yellow epidermis, easily deciduous when dry. Color white, except that the larger ribs are alternately light brown and white, and the apex, consisting of about three smooth nuclear whorls, is dark brown. Length 68^{mm}; breadth 56^{mm}; length of aperture 53^{mm}.

The animal is well preserved. Proboscis blackish, exserted about 20^{mm}, thick (8^{mm}) and clavate at the end, which is surrounded by a sort of collar, with a finely wrinkled or crenulated, white edge. Head large, with a prominent rounded lobe in front. Tentacles large, elongated (10^{mm}), stout, tapering, obtuse. Eyes small, black, on distinct, but slightly raised tubercles at the outer base of the tentacles. Head, tentacles and siphon-tube dull brown. Penis very large (50^{mm} long, 12^{mm} broad), twisted and thickened at base, flattened distally, terminating in a slightly prominent obtuse lobe at the tip; a well-marked groove runs along the posterior edge to the tip.

Off Martha's Vineyard, station 945; 202 fathoms. Station 1036; 94 fathoms; one young specimen and large fragments.

Pleurotoma (*Bela*) *limacina* Dall. (*Daphnella?*)

Bulletin Mus. Comp. Zool., ix, p. 55, 1881.

Four living specimens of this elegant shell were taken at station 994; 368 fathoms. Gulf of Mexico, 447–805 fathoms (Dall). This is not a true *Bela*, for it has no operculum; eyes minute.

Capulus hungaricus (Linné).

Two living specimens were obtained, which appear to belong to this species. They are more delicate and have somewhat finer and more regular radiating ribs than the ordinary European form. It has not been recorded before from our coast.

Stations 922, 1029; 69 and 458 fathoms.

Fiona nobilis Alder and Han.

British Nud. Moll., Æolidæ, Fam. 3, pl. 38A.

A large and handsome *Fiona*, apparently this species, was found in two instances, in large numbers, on pieces of floating timber, among Anatifers, at stations 935 and 995. They were kept in confinement several days and laid numerous clusters of eggs. These are in the form of a broad ribbon, spirally coiled in about one and a half turns, so to form a bell-shaped or cup-shaped form, and attached by a slender pedicel, so as to hang from the under sides of objects. Alder and Hancock recorded its occurrence, in a single instance, at Falmouth, England.

Issa ramosa Verrill and Emerton, sp. nov.

Body elevated, convex above, elongated, oblong, sides nearly parallel along the middle; foot well-developed, as broad as the body. Dorsal tentacles thick, clavate, obtuse, with numerous lamellæ; sheath scarcely raised. Back and sides with numerous small, simple papillæ. Along the lateral margins of the back there is a carina, with a row of large, much branched papillæ, alternating with much smaller ones; of the large ones there are about six on each side, the most anterior are below the dorsal tentacles; two on each side are posterior to the gills, the last ones largest; a row of similar but smaller processes extends below the tentacles and around the front margin.

Gills five, arborescently branched. Color, pale yellow. The dorsal tentacles darker.

The radula is quite different from that of *I. lacera* and *Triopa claviger*. The median area is wide, with two rows of thin, transversely oblong plates; there are three rows of large, nearly equal teeth on each side, with the tips strongly incurved, obtuse; the innermost tooth has a small lobe on the middle of the inner edge; these are followed by about seventeen or eighteen smaller, oblong plates, with slightly emarginate anterior ends; these gradually decrease in size toward the margins of the radula.

Stations 940, 949; 130 and 100 fathoms.

In form, this resembles *I. lacera*, but is easily distinguished by the branched appendages along the sides.

Of the Lamellibranchiata, some very interesting new forms occurred. The most important of these are species of *Pholadomya*, *Mytilimeria* and *Diplodonta*,—three genera not before found on this coast. The *Pholadomya* is more related to certain fossil forms than to any of the few described living species. The genus *Mytilimeria* has hitherto had very few living representatives, and none of them resemble our very singular species.

Among the northern forms, not previously found south of Cape Cod, are the following: *Mya truncata; Spisula ovalis* (975, 976, 981); *Leda tenuisulcata* (973); *Nucula tenuis.*

Pholadomya arata Verrill and Smith, sp. nov.

Shell triangular, short, wedge-shaped, posterior end angular, somewhat produced, obtuse; anterior end very short and abruptly truncated, clearly defined by a carina extending from the beak to the outer margin; anterior to the carina there is a broad concave furrow, which bounds the slightly convex central area of the front end; the greater part of the sides of the shell is covered with deep, rather wide, concave furrows, separated by elevated, sharp-edged ribs; the furrows vary in width and decrease

posteriorly; a small portion, near the tip of the posterior end is covered only by slight ribs. The surface between the ribs is finely granulated. When the thin superficial layer is removed the surface is pearly. The umbos are prominent, strongly incurved, nearly or quite in contact. The hinge in the right valve consists of a small, slightly prominent lamella, running back as a low ridge, and separated from the margin of the shell anteriorly, and from the cartilage-lamina posteriorly, by a narrow groove; the cartilage-pit is long, running forward under the beak as a a narrow furrow; it is bounded internally by a prominent lamella. Length, 36^{mm}; height, 29^{mm}; breadth, 26^{mm}.

Stations 940, 949, 950: 69 to 130 fathoms.

Three specimens, all dead, but one is very fresh.

Mytilimeria flexuosa Verrill and Smith, sp. nov.

Shell obliquely cordate, short, higher than long, very swollen, the anterior end rather shorter than the posterior; umbos very prominent, beaks much incurved, pointed and turned forward, with a small, deep concavity just under and in front of them. The outline and surface of the shell is very flexuous, owing to the broad deep grooves and elevated ribs which divide the surface into several areas. The most prominent rib is very high and rounded, and runs from the beak to the extreme ventral margin, inclining somewhat forward; in front of this the anterior area is flattened with a wide shallow concave groove or undulation in the middle, and others less marked; the front edge is broadly rounded, slightly undulated below. The middle area is very elevated, and forms more than a third of the shell; it is flattened or slightly concave in the middle, and undulated by several faint broad ribs; it recedes posteriorly, and a broad concave furrow separates it from the small posterior area, which is without ribs, and has a prominent rounded edge. The surface is finely granulated, lines of growth evident. The interior is pearly, angulated by a deep groove, corresponding to the largest external rib. The dorsal hinge-line is nearly straight posteriorly, and strongly incurved anteriorly, in the right valve it projects inward, but not in the left; in the right valve there is a small rounded tubercle, a little back of the beak; from below this a short rib-like process runs back below the deep, partially internal cartilage-pit, which extends forward and upward under the beak as a narrow furrow. Anterior muscular scar deep; posterior one larger ovate, less distinct; sinus small. Length, 25^{mm}; height, 26^{mm}; breadth from side to side, 22^{mm}.

Station 947; 312 fathoms. One pair of fresh valves, dead.

This and the preceding were both taken by means of the "rake-dredge."

Diplodonta turgida **Verrill and Smith, sp. nov.**

Shell large for the genus, round-ovate, a little longer than high, very swollen; the two ends nearly equally rounded, the anterior a little narrower; ventral edge broadly and regularly rounded; beaks nearly central, somewhat forward of the middle, strongly curved inward and forward, acute. Surface without sculpture, smooth except for the evident lines of growth. In the right valve there are, opposite the beak, two nearly equal, stout, sharp teeth, separated by a space of about the same width; back of these, and partly joined at base to the posterior one, there is a much larger, broad, stout, obtuse tooth, with a groove on its dorsal side; external cartilage-groove and its lamella are long and narrow, curved. Length, 29^{mm}; height (umbos to ventral edge), 25^{mm}; breadth, 23^{mm}.

Station 950; 69 fathoms. One right valve.

ART. XLII.—*Note on the Tail of Comet b*, 1881; by LEWIS BOSS. With Plates V and VI.

THE changes which took place in the aspect of the tail of the great comet of 1881, during the last days of June, seemed to me of peculiar and unusual interest. Appearances so novel and unexpected moved me to prepare some rude sketches of the tail, with brief notes as to its position in the sky. From several causes my opportunities for making such studies proved to be very few, and lack of experience contributed to diminish the completeness and accuracy of the results actually obtained. It is to be regretted that the number of those who give serious and systematic attention to this branch of observation is quite small in view of the small number of opportunities; while, on the other hand, the observations which can be made are uncertain in character, and the results vary much with individual judgment. It is therefore important that drawings and descriptions should be gathered from as many sources as possible.

The engravings (Plate V), accompanying this paper were reduced from drawings compiled from the original sketches and notes.

These were made in the open air at the times of observation indicated. In all cases the chief object of interest was what may be conveniently termed the right-line tail, which was far more conspicuous than the other branch on June 26, scarcely perceptible on June 28, and entirely wanting on July 1. It is to be regretted that on these dates charts were not used in the preparation of the original sketches, except for reference. The final drawings were laid down on copies of Schwinck's polar

chart (1850) from the original sketches and notes. On July 22 the outlines of the tail were drawn with care on the *Durchmusterung* polar chart (Argelander, 1855), and from thence accurately transferred to the finished sketch. The distortion of figure, owing to the projection used, is not important in any case, and for the purposes of this communication it is inappreciable. The engraver has been very successful in preserving the accuracy of the original drawings, and in imparting to them the desired effects. The following is the substance of the notes recorded:

June 26, 10^h.—Air wonderfully transparent. The tail of the great comet consists of two branches. The principal branch appears to be perfectly straight, and passes about two degrees to the apparent east of Polaris and eight or ten degrees beyond it. For the last ten or fifteen degrees this branch is exceedingly faint. The other is curved quite strongly to the apparent west, and after its separation from the principal ray requires most careful scrutiny for its detection. It seems to extend to a point six or seven degrees, astronomically southeast from Polaris.

June 26, 13^h 30^m. Sketch.—The tail presents to the naked eye much the same appearance as it did earlier in the evening, except that neither branch can be traced so far as then seen. The straight branch appears to pass quite centrally over 2 Ursæ Minoris, and to extend about two degrees beyond B. A. C. 7851. Its breadth seems to be nearly uniform and a little more than one degree. With the aid of a straight edge no curvature could be safely assigned. There is a rather sudden falling off in brightness at a point four or five degrees from 2 Ursæ Minoris toward the nucleus. The edges of this ray are ill-defined and the central parts brightest. The ray which curves toward greater right ascension is not satisfactorily seen. Its effect is to broaden and intensify the principal ray for a distance from the nucleus equal to about four-tenths the whole distance to Polaris. At this point the total breadth of the tail is estimated to be about four degrees. Here a separation is faintly indicated, but the continuation of the curved ray is observed with extreme difficulty. The direction and extent of this branch is indicated on the sketch.

June 28, 13^h. Sketch.—Foggy haze low down in the north. Sky otherwise satisfactory. The nearly straight ray described on June 26 has dwindled to a faint and narrow streak, which might have been overlooked, had not a bright one been expected in its place. It extends to a point near 2 Ursæ Minoris as indicated in the sketch. Its breadth is not over one-third of a degree. The curved branch is brightest in its central parts, and is very conspicuous for the first ten or fifteen degrees of its length. It seems to terminate about three degrees short of B. A. C. 4349; though at times a much greater extent is suspected. Fifth magnitude star (B. A. C. 2326) is 15′ inside the following edge of the tail. The axis of this branch passes to the apparent east of

B. A. C. 4349, and at a distance from it equal to about one-fifth the distance between that star and Polaris. The last direction of the axis is toward β Ursæ Minoris. The distance of Polaris from the preceding edge of the tail is nearly equal to the distance between Polaris and 2 Ursæ Minoris. The breadth at three-fourths the distance from the nucleus is about three degrees.

July 1, 12^h 15^m. Sketch.—State of sky not remarkably fine. The tail is much shorter than heretofore, and its appearance entirely changed. There is no trace of the straight ray seen on June 26 and 28. The preceding edge of the tail appears nearly straight. It is brighter and extends to a greater distance from the nucleus than the following edge. The latter is strongly curved near the end. The breadth is about three degrees at the widest part.

July 13, 10^h 15^m.—Tail single, faint, and diffuse. Estimated length seven degrees. Breadth near the end, about 40′. The direction of the axis prolonged passes to the east of ε Ursæ Minoris, at a distance about one fifth that between ε and δ Ursæ Minoris.

July 22, 14^h. Sketch.—Four-inch Clark Comet seeker. Power twelve. Field 2° 30′. Sky fine. Two branches seen. The first is nearly straight and brighter than the other. Estimated width 10′. This branch is certainly recognized as far as A. R. 14^h 20^m. Sometimes I imagine that it extends as far as A. R. 15^h 40^m. [As indicated by the dotted line in diagram.] The light seems to be composed of a great number of parallel bright streaks. This appearance of striation is very decided in the region within two degrees of the nucleus. The southern branch is curved and much shorter and fainter than the straight ray. The location of the last degree of length represented in the sketch is very difficult. The breadth here is estimated to be 30′ or 40′. The bounding lines are carefully laid in on the *Durchmusterung* chart, and their position relatively to stars frequently compared with the sky during the progress of the sketch. Sky suddenly clouded at 14^h 30^m.

During the remainder of July the appearance of the tail did not essentially change. I was absent from the observatory for a short time in the early part of August, and did not again obtain a telescopic view of the tail until August 17. It was then apparently single. The estimated length was 3°. There are slight inconsistencies in the notes of June 28, which have been adjusted according to the supposed weights of the various estimations.

For the points most carefully determined, and with such approximation as appears to be warranted by the precision of the observations, we have for positions of points in the tails on the respective dates:

TABLE I.

	Nucleus.		Axis, right-line tail.		Curved tail.		Point in curved tail observed.
	a	δ	a	δ	a	δ	
June 26, 13^h 30^m	87°·2	57°·9	316°	83°·0	99°	80°·5	Axis.
	----	----	13·2	85·6	----	----	
June 28, 13^h 00	90·1	63·9	20·	85·9	155	86·0	Axis.
	----	----	----	----	100	83·0	Axis.
July 1, 12^h 15^m	95·8	70·7	----	----	111·2	87·0	Prec. edge.
	----	----	----	----	115·3	80·0	Foll. edge.
July 22, 14^h 00	177·6	81·9	215·4	82·8	205·0	82·2	Axis.

It would have been better, no doubt, to have made no special effort to determine the position of the extreme visible limit of the tail, but to have given greater attention to the position of the axis and the breadth of the visible portions at points where the tail could be easily seen. But even with the present imperfect data, we shall be able to derive some idea of the real position of the tails in space, and of their correspondence in type with others which have been observed.

Convenient formulæ have been devised by Bessel (Astr. Nachr., vol. xiii, p. 193), by the use of which we may determine the angular deviation of a point in the tail from the radius vector prolonged. It will be necessary to assume that the axis of the tail lies in the plane of the orbit of the nucleus. This assumption is well supported both by theory and experience, and is, no doubt, substantially correct. Such small deviations as might result when emissions of matter from the head are unsymmetrical with reference to the orbit plane, or when the initial velocity of particles thrown off from the nucleus is greater toward one pole of the orbit than toward the other, may probably be neglected as comparatively insignificant. Let:

r=Radius vector of the nucleus at the time of observation.
ρ=Geocentric distance of nucleus.
Δ=Length of tail, or distance of point observed from the nucleus.
s=Angular length of tail.
p^0=Position angle at the nucleus of r prolonged.
p=Corresponding angle for the observed point in the tail.
S=The cometocentric distance of the earth from the north pole of the comet's orbit.
T=Cometocentric angle between the earth and the observed point in the tail.
φ'=The cometocentric angle between the observed point and the radius vector prolonged,—positive, when this point is on that side of the radius vector from which the comet has been moving.

From the elements of Dr. Oppenheim (Astr. N., 2384), we find for the coördinates of the north pole of the orbit of comet b, referred to the equator,

$$A=192^\circ\ 09'. \qquad D= +23^\circ\ 46'.$$

We then derive the following table of results:

TABLE II.

	June 26.			June 28.			July 1.		July 22.	
	Axis of right-line tail at end.	Axis of right-line tail at 2 Urs. Min.	Axis of curved tail at end.	Axis of right-line tail at end.	Axis of curved tail at end.	Axis of curved tail near B. A. C. 2326.	Preceding edge at end.	Following edge at end.	Axis of right-line tail at end.	Axis of curved tail at end.
r	·763	----	----	·775	----	----	·795	----	1·018	----
ρ	·340	----	----	·374	----	----	·428	----	·853	----
Δ	·210	·187	·179	·161	·189	·153	·130	·110	0·82	·057
s	37°·0	31°·1	22°·9	25°·0	24°·7	19°·2	16°·4	10°·4	5°·0	3°·9
$p°$	345·6	----	----	348·6	----	----	353·1	----	57·7	----
p	351·2	351·8	5·0	350·8	8·7	3·7	2·8	18·6	61·5	71·8
S	102·9	----	----	106·1	----	----	110·3	----	121·6	----
T	40·2	39·1	24·9	54·0	30·0	34·4	51·8	32·9	109·8	94·1
ϕ'	12·9	14·0	29·8	5·6	32·1	27·0	21·1	41·5	6·2	24·9
							31·3			

An inspection of the foregoing table shows that the characteristics of the two branches of the tail, as defined by the values of φ', present a similarity quite as striking as could have been predicted in view of the considerable probable errors to which such determinations are liable. On the first three dates the cometocentric elevation of the earth above the plane of the comet's orbit was, respectively, 13°, 16°, and 20° only; so that small errors in the observed position angle are considerably multiplied when converted into the corresponding values of φ'. It must also be remembered that many of the points observed are several degrees farther from the nucleus than the superior limit of visibility assigned by most observers for the extent of the tail on the respective dates.

So far as I am aware most of the observers who have already reported on the appearance of the tail failed to notice the division into branches at all. On the other hand, it cannot be supposed that this interesting aspect entirely escaped detection under proper conditions of sky and terrestrial surroundings.

If we examine similar computations which have been made on the tails of other great comets we see that the two branches resemble the two types most frequently observed. The right-line tail corresponds to the principal appendages of the great comets of 1811, 1835 (Halley's), 1843, 1861, 1862, and others. The general direction also conforms to that of the secondary tail of the great comets of 1858, 1874 and others; but in the present case the light of this tail is relatively far more conspicuous. The branch of greater curvature finds its representative in the great majority of comets which have been observed.

The tail of the comet of 1807 presents most striking resemblance to this under discussion. On October 22, 1807, the comet of that year had, generally speaking, the same position in space as the present comet had on July 22. On that occasion (Astr. Nachr., vol. xiii, p. 228), Bessel found two tails. The first he considered to be nearly straight and in length about 4·5°. The other was strongly curved, broader than the first, and in length about 3°. Dr. Bredichin (Mosc. Ann., vol. v, pt. 2, p. 56), has computed the value of φ' for the end of each tail. This enables us to compare the two descriptions in a very satisfactory manner. We have—

	Comet of 1807.			Comet of 1881.		
	Δ	ρ'	s	Δ	ρ'	s
For the right-line tail, - -	·139	7°·9	4°·5	·082	6°·2	5°·0
For the curved tail, - - -	·105	24·2	3·0	·057	24·9	3·9

Allowing for the difference in values of $\varDelta$ and r, the agreement is quite within the probable errors of observation. It is thus seen that there is great similarity in the physical appearance of the two comets, as well as between the elements of their respective orbits. Since, in general, we have the greatest possible variety in the appearance of the tails of the comets, and especially in the combination of tails of different types, we may confidently say, that the very remarkable similarity above shown furnishes another important fact, in addition to those which already tend to indicate a common origin for the comets of 1807 and 1881.

Sir Isaac Newton and others after him have shown that the tail might be produced by a repulsive force emanating from the sun, and acting on detached particles, which are continually thrown out from the nucleus of all great comets. Bessel has investigated formulæ (Astr. Nachr., vol. xiii) which enabled him to compute the repulsive force necessary to produce a tail of the form actually observed in the case of Halley's comet. The repulsive force in these formulæ is, of course, an implicit function. Bessel's formulæ are shown (Mosc. Ann., vol. v, pt. 2) to give results which are but roughly approximate for large distances from the nucleus. Professor Norton, Dr. Bredichin and others have published formulæ which are more rigorously exact. In all these investigations it is supposed that a particle projected from the nucleus is repelled by a force $(1-\mu)$ the reverse of the Newtonian. The effective force acting on the particle will be μ, and when combined with the tangential velocity of the nucleus will cause it to describe a hyperbolic orbit. This hyperbola will be convex or concave to the sun, according as $(1-\mu)$ is greater or less than unity. In the volumes of the Moscow Annals, Dr. Bredichin presents a variety of reasearches concerning the consequences to be deduced from this assumption of repelling forces.

He refers the tails of comets to three general types, distinguished by the value of $(1-\mu)$ employed in their theoretical representation. The value of $(1-\mu)$ (expressed in the Newtonian unit) for Type I is 11·0 to 12·0; for Type II, about 1·3; for Type III, 0·3, or less. The value of $(1-\mu)$ for Type II, however, is found to vary considerably in different cases without losing its distinctive character. It is possible to introduce the effect due to the initial velocity of projection from the nucleus, and this, of course, modifies the value of $(1-\mu)$ which would otherwise be assumed. This effect will evidently be proportionally least in tails of Type I, and will increase in importance as the value of $(1-\mu)$ is diminished. If we suppose particles to be projected from the nucleus equally in all directions with equal velocities, the effect will be mainly shown in the breadth of the tail. Thus we invariably find tails of Type I to be narrow in comparison with those of Type II,—a fact which finds satisfactory explanation in the relatively small effect, which would be produced by the action of initial velocity, when the repelling force is relatively very great. But since cometary emissions appear to take place mostly on the side of the nucleus nearest the sun, the assumption of the value zero for initial velocity will always render the value of $(1-\mu)$ computed from observation, too small.

It will be interesting to examine our observations of the tail of comet *b* 1881, with a view to determining to what extent they conform to the normal types. In a preliminary discussion like this, which is founded on few observations of small weight, it will not be worth while to include the effect of initial velocity of emission. When a great number of observations of the tail and coma have been collected, it may be possible to arrive at some satisfactory result in this direction. I have accordingly computed the hyperbolic orbits of particles emitted from the nucleus at various times (previous to the observations on the tail), with values of $(1-\mu)$ equal to ·6, 1·0, 1·4 and 11·0. The values of the radius vector and true parabolic anomaly of the nucleus have been computed from the elements of Dr. Oppenheim, previously cited.

Let:

M = Date when a given particle is observed in the tail.
M′ = Time of emission of that particle from the nucleus.
M″ = Perihelion passage of the particle.
E = Eccentricity of the hyperbolic orbit.
I = Angle between the radii vectores of the particle and nucleus at the time M. For the particle referred to the nucleus, this angle will evidently always be retrograde to the motion of the nucleus.
Δ = Distance of the particle from the nucleus at the time, M.

η = Length of perpendicular let fall from the particle on r produced at the time, M.

ξ = Distance from the foot of that perpendicular to the nucleus.

φ = Angle whose sine is $\frac{\eta}{\varDelta}$, or the angle between r prolonged and the line joining the nucleus and particle at the time M.

As an example of the manner in which the theoretical lines of Plate VI have been constructed, the results of computations intended to represent the right-line tail of June 26·805 (Berlin time) are subjoined. The value of $(1-\mu)$ is assumed to be 11·0; and the hyperbolical orbits are computed for particles emitted at perihelion, and for two designated dates subsequent to that time. We have:

M′	June 16·510	June 18·510	June 20·510
M″	June 16·510	June 18·359	June 20·205
log E	0·0792	0·0791	0·0788
I	4° 15′	2° 29′	1° 13′
$\varDelta$	·284	·192	·110
ξ	·273	·187	·109
η	·077	·041	·018
φ	15°·7	12°·4	9°·5

From the values of $\varDelta$, ξ, and η, the curve marked I in the figure for June 26 (Pl. VI) is constructed. From that curve we derive by a graphic process the values of φ corresponding to the observed values of $\varDelta$ at two points in the tail on that date. We thus have:

	Δ	ϕ'	ϕ
June 26·805 Type I	·210	12°·9	13°·2
	·187	14·0	12·3

The agreement between the values of φ' and φ is even closer than could have reasonably been expected from the unavoidable probable error in the determination of φ'.

In the diagrams of Plate VI, the point N represents the position of the nucleus at the respective times of observation. N R′ is the radius vector prolonged. The curves N I are carefully constructed in the original diagrams from the computed positions of two or more particles, when $(1-\mu)=11·0$. The previous dates of emission were so chosen that one or more computed points would fall near that which was actually observed. The curves N II were constructed with $(1-\mu)=1·4$, and may represent the tail of Type II. The intervals between dates of emission and observation for like values of $\varDelta$ are much greater in this case than in that for tails of Type I. The curves N II″ are constructed with $(1-\mu)=1·0$; and N III″ for July 22, is based on $(1-\mu)=0·6$. The dots enclosed in small circles indicate the positions of points in the tail actually observed.

The computed positions of these are given in table II. The dotted lines are intended to give a rough idea of the outlines of the tail as observed and reduced to the plane of the orbit, on the somewhat doubtful assumption that the thickness of the tail may be neglected in comparison with its breadth in the plane of the orbit. Following is a tabular view of the results obtained by computation, with the corresponding values from observation.

TABLE III.

Date.	Type I. $(1-\mu) = 11{\cdot}0$.					Type II.					
	Point	Δ	ϕ'	ϕ	$\phi-\phi'$	Point	Δ	ϕ'	ϕ	$\phi-\phi'$	$(1-\mu)$
			°	°	°			°	°	°	
June 26	I'_1	·210	12·9	13·2	+ ·3	II'	·179	29·8	31·2	+1·4	1·4
	I'_2	·187	14·0	12·3	−1·7						
June 28	I'	·161	5·6	11·0	+5·4	II'_1	·189	32·1	32·8	+ ·7	1·4
						II'_2	·153	27·0	29·7	+2·7	1·4
July 1						II'_1, II'_2	·120	31·3	26·5	−4·8	1·4
July 22	I'	·082	6·2	5·7	− ·5	II'	·057	24·9	12·	−13·	1·4
									16·	− 9·	1·0
									21·	− 4·	·6

A value of 0·4 for $(1-\mu)$ would give a fair approximation to the tail of Type II as observed on July 22. The agreement of the observed and the computed values of φ for the tail of the first type is very satisfactory. The deviation of five degrees on June 28 might easily be attributed to errors of observation on an object which was so excessively faint; and it is quite probable that the location of the end point was somewhat influenced by the general direction of the tail nearer the nucleus where it was brighter. Such an influence would tend to make the observed value of φ too small. The two values of φ' best determined for Type I are the second and fourth of the table; and these both indicate that a smaller value of $(1-\mu)$ should have been employed.

With reference to the comparisons of observed and computed φ in the tail of the second type, we do not expect an accordance so satisfactory. The difficulties of observation were greater with this branch of the tail, which was broad and faint at its extremity; and, furthermore, an error in location of this shorter branch would have a greater influence upon the value of φ'. The probable uncertainty in the value of φ' for the first three dates I estimate at three or four degrees. On July 22 the location of the shorter branch of the tail was extremely difficult; still I cannot think that the probable uncertainty in φ' is greater than four or five degrees. This would make any value of $(1-\mu)$

posteriorly; a small portion, near the tip of the posterior end is covered only by slight ribs. The surface between the ribs is finely granulated. When the thin superficial layer is removed the surface is pearly. The umbos are prominent, strongly incurved, nearly or quite in contact. The hinge in the right valve consists of a small, slightly prominent lamella, running back as a low ridge, and separated from the margin of the shell anteriorly, and from the cartilage-lamina posteriorly, by a narrow groove; the cartilage-pit is long, running forward under the beak as a a narrow furrow; it is bounded internally by a prominent lamella. Length, 36^{mm}; height, 29^{mm}; breadth, 26^{mm}.

Stations 940, 949, 950: 69 to 130 fathoms.

Three specimens, all dead, but one is very fresh.

Mytilimeria flexuosa Verrill and Smith, sp. nov.

Shell obliquely cordate, short, higher than long, very swollen, the anterior end rather shorter than the posterior; umbos very prominent, beaks much incurved, pointed and turned forward, with a small, deep concavity just under and in front of them. The outline and surface of the shell is very flexuous, owing to the broad deep grooves and elevated ribs which divide the surface into several areas. The most prominent rib is very high and rounded, and runs from the beak to the extreme ventral margin, inclining somewhat forward; in front of this the anterior area is flattened with a wide shallow concave groove or undulation in the middle, and others less marked; the front edge is broadly rounded, slightly undulated below. The middle area is very elevated, and forms more than a third of the shell; it is flattened or slightly concave in the middle, and undulated by several faint broad ribs; it recedes posteriorly, and a broad concave furrow separates it from the small posterior area, which is without ribs, and has a prominent rounded edge. The surface is finely granulated, lines of growth evident. The interior is pearly, angulated by a deep groove, corresponding to the largest external rib. The dorsal hinge-line is nearly straight posteriorly, and strongly incurved anteriorly, in the right valve it projects inward, but not in the left; in the right valve there is a small rounded tubercle, a little back of the beak; from below this a short rib-like process runs back below the deep, partially internal cartilage-pit, which extends forward and upward under the beak as a narrow furrow. Anterior muscular scar deep; posterior one larger ovate, less distinct; sinus small. Length, 25^{mm}; height, 26^{mm}; breadth from side to side, 22^{mm}.

Station 947; 312 fathoms. One pair of fresh valves, dead.

This and the preceding were both taken by means of the "rake-dredge."

lodonta turgida Verrill and Smith, sp. nov.

hell large for the genus, round-ovate, a little longer than ı, very swollen; the two ends nearly equally rounded, the ›rior a little narrower; ventral edge broadly and regularly ıded; beaks nearly central, somewhat forward of the mid- strongly curved inward and forward, acute. Surface with- sculpture, smooth except for the evident lines of growth. he right valve there are, opposite the beak, two nearly equal, t, sharp teeth, separated by a space of about the same th; back of these, and partly joined at base to the posterior there is a much larger, broad, stout, obtuse tooth, with a ›ve on its dorsal side; external cartilage-groove and its ella are long and narrow, curved. Length, 29^{mm}; height bos to ventral edge), 25^{mm}; breadth, 23^{mm}.

tation 950; 69 fathoms. One right valve.

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r. XLII.—*Note on the Tail of Comet b*, 1881; by Lewis Boss. With Plates V and VI.

'HE changes which took place in the aspect of the tail of the ıt comet of 1881, during the last days of June, seemed to of peculiar and unusual interest. Appearances so novel unexpected moved me to prepare some rude sketches of tail, with brief notes as to its position in the sky. From ›ral causes my opportunities for making such studies ved to be very few, and lack of experience contributed to inish the completeness and accuracy of the results actually ıined. It is to be regretted that the number of those who › serious and systematic attention to this branch of obser- on is quite small in view of the small number of opportu- ›s; while, on the other hand, the observations which can nade are uncertain in character, and the results vary much ı individual judgment. It is therefore important that wings and descriptions should be gathered from as many rces as possible.

'he engravings (Plate V), accompanying this paper were uced from drawings compiled from the original sketches notes.

'hese were made in the open air at the times of observation icated. In all cases the chief object of interest was what y be conveniently termed the right-line tail, which was far re conspicuous than the other branch on June 26, scarcely ceptible on June 28, and entirely wanting on July 1. It is ›e regretted that on these dates charts were not used in the paration of the original sketches, except for reference. The ıl drawings were laid down on copies of Schwinck's polar

chart (1850) from the original sketches and notes. On July 22 the outlines of the tail were drawn with care on the *Durchmusterung* polar chart (Argelander, 1855), and from thence accurately transferred to the finished sketch. The distortion of figure, owing to the projection used, is not important in any case, and for the purposes of this communication it is inappreciable. The engraver has been very successful in preserving the accuracy of the original drawings, and in imparting to them the desired effects. The following is the substance of the notes recorded:

June 26, 10^h.—Air wonderfully transparent. The tail of the great comet consists of two branches. The principal branch appears to be perfectly straight, and passes about two degrees to the apparent east of Polaris and eight or ten degrees beyond it. For the last ten or fifteen degrees this branch is exceedingly faint. The other is curved quite strongly to the apparent west, and after its separation from the principal ray requires most careful scrutiny for its detection. It seems to extend to a point six or seven degrees, astronomically southeast from Polaris.

June 26, 13^h 30^m. Sketch.—The tail presents to the naked eye much the same appearance as it did earlier in the evening, except that neither branch can be traced so far as then seen. The straight branch appears to pass quite centrally over 2 Ursæ Minoris, and to extend about two degrees beyond B. A. C. 7851. Its breadth seems to be nearly uniform and a little more than one degree. With the aid of a straight edge no curvature could be safely assigned. There is a rather sudden falling off in brightness at a point four or five degrees from 2 Ursæ Minoris toward the nucleus. The edges of this ray are ill-defined and the central parts brightest. The ray which curves toward greater right ascension is not satisfactorily seen. Its effect is to broaden and intensify the principal ray for a distance from the nucleus equal to about four-tenths the whole distance to Polaris. At this point the total breadth of the tail is estimated to be about four degrees. Here a separation is faintly indicated, but the continuation of the curved ray is observed with extreme difficulty. The direction and extent of this branch is indicated on the sketch.

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B. A. C. 4349, and at a distance from it equal to about one-fifth the distance between that star and Polaris. The last direction of the axis is toward β Ursæ Minoris. The distance of Polaris from the preceding edge of the tail is nearly equal to the distance between Polaris and 2 Ursæ Minoris. The breadth at three-fourths the distance from the nucleus is about three degrees.

July 1, 12^h 15^m. Sketch.—State of sky not remarkably fine. The tail is much shorter than heretofore, and its appearance entirely changed. There is no trace of the straight ray seen on June 26 and 28. The preceding edge of the tail appears nearly straight. It is brighter and extends to a greater distance from the nucleus than the following edge. The latter is strongly curved near the end. The breadth is about three degrees at the widest part.

July 13, 10^h 15^m.—Tail single, faint, and diffuse. Estimated length seven degrees. Breadth near the end, about 40′. The direction of the axis prolonged passes to the east of ε Ursæ Minoris, at a distance about one fifth that between ε and δ Ursæ Minoris.

July 22, 14^h. Sketch.—Four-inch Clark Comet seeker. Power twelve. Field 2° 30′. Sky fine. Two branches seen. The first is nearly straight and brighter than the other. Estimated width 10′. This branch is certainly recognized as far as A. R. 14^h 20^m. Sometimes I imagine that it extends as far as A. R. 15^h 40^m. [As indicated by the dotted line in diagram.] The light seems to be composed of a great number of parallel bright streaks. This appearance of striation is very decided in the region within two degrees of the nucleus. The southern branch is curved and much shorter and fainter than the straight ray. The location of the last degree of length represented in the sketch is very difficult. The breadth here is estimated to be 30′ or 40′. The bounding lines are carefully laid in on the *Durchmusterung* chart, and their position relatively to stars frequently compared with the sky during the progress of the sketch. Sky suddenly clouded at 14^h 30^m.

During the remainder of July the appearance of the tail did not essentially change. I was absent from the observatory for a short time in the early part of August, and did not again obtain a telescopic view of the tail until August 17. It was then apparently single. The estimated length was 3°. There are slight inconsistencies in the notes of June 28, which have been adjusted according to the supposed weights of the various estimations.

For the points most carefully determined, and with such approximation as appears to be warranted by the precision of the observations, we have for positions of points in the tails on the respective dates:

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Velocity of Light.*—Lord RALEIGH discusses the recent paper of Young and Forbes (Roy. Soc. Proc., May 17, 1881), in which it is maintained that blue light travels *in vacuo* about 1·8 per cent faster than red light, and asks the question: what is really determined by observations on the velocity of light? Is the velocity of a single wave determined, or that of a group of waves? If the group velocity be denoted by U and the wave velocity by V, the relation between these velocities is explained by $U=\frac{d\,(kV)}{d\,k}$, in which k is inversely proportional to the wave length. According to Young and Forbes, V varies with k and therefore U and V are different. A complete knowledge of U, which can be obtained by experiment, does not lead to a knowledge of V. Lord Rayleigh discusses the various methods employed in determining the velocity of light and concludes that if we regard the solar parallax as known, we obtain almost the same velocity of light from the eclipses of Jupiter's satellites as from observation, although the first result relates to the group velocity and the second to the wave velocity. There cannot be, therefore, a difference of two or three per cent between the group velocity and the wave velocity. These considerations lead Lord Rayleigh to doubt the conclusions of Young and Forbes.—*Nature*, Aug. 25, 1881, p. 382. J. T.

2. *Movement of Sound Waves in Organ Pipes.*—Dr. RUDOLPH KOENIG has contrived an ingenious arrangement which enables one to observe the nodes and segments of a sound wave in its passage through an organ pipe. The pipe is slotted along its entire side, is then placed in a horizontal position with the slot beneath and resting in a trough of water. The water thus forms a portion of the lower side of the pipe and the slot allows a hollow glass tube, U-shaped, to be pushed along the interior throughout its entire length. By connecting the glass tube with manometric capsules, one can discover the position of the nodes and also observe peculiarities in the movements of the waves.—*Ann. der Physik und Chemie*, No. 8, 1881. J. T.

3. *On the Conductivity of Metals for Heat and Electricity.*—In the continuation of a paper on this subject, Herr L. Lorenz discusses the theoretical laws of the cooling of metals when placed in ordinary air and extends his observations to the conduction of heat by metals in general. If T represents the absolute temperature, k and x the conductivity for heat and electricity respectively, he is led to the following expression: $\frac{k}{x}=T\times$ constant. According to his view there is discontinuity in the interior of every body and there are regions or sections along which free electricty can move without manifesting difference of

We then derive the following table of results:

TABLE II.

	June 26.			June 28.			July 1.		July 22.	
	Axis of right-line tail at end.	Axis of right-line tail at 2 Urs. Min.	Axis of curved tail at end.	Axis of right-line tail at end.	Axis of curved tail at end.	Axis of curved tail near B. A. C. 2326.	Preceding edge at end.	Following edge at end.	Axis of right-line tail at end.	Axis of curved tail at end.
r	·763	----	----	·775	----	----	·795	----	1·018	----
ρ	·340	----	----	·374	----	----	·428	----	·853	----
Δ	·210	·187	·179	·161	·189	·153	·130	·110	0·82	·057
s	37°·0	31°·1	22°·9	25°·0	24°·7	19°·2	16°·4	10°·4	5°·0	3°·9
p°	345·6	----	----	348·6	----	----	353·1	----	57·7	----
p	351·2	351·8	5·0	350 8	8·7	3·7	2·8	18·6	61·5	71·8
S	102·9	----	----	106·1	----	----	110·3	----	121·6	----
T	40·2	39·1	24·9	54·0	30·0	34·4	51·8	32·9	109·8	94·1
ϕ'	12·9	14·0	29 8	5·6	32·1	27·0	21·1	41·5	6·2	24·9
							$\underbrace{\qquad}_{31·3}$			

An inspection of the foregoing table shows that the characteristics of the two branches of the tail, as defined by the values of φ', present a similarity quite as striking as could have been predicted in view of the considerable probable errors to which such determinations are liable. On the first three dates the cometocentric elevation of the earth above the plane of the comet's orbit was, respectively, 13°, 16°, and 20° only; so that small errors in the observed position angle are considerably multiplied when converted into the corresponding values of φ'. It must also be remembered that many of the points observed are several degrees farther from the nucleus than the superior limit of visibility assigned by most observers for the extent of the tail on the respective dates.

So far as I am aware most of the observers who have already reported on the appearance of the tail failed to notice the division into branches at all. On the other hand, it cannot be supposed that this interesting aspect entirely escaped detection under proper conditions of sky and terrestrial surroundings.

If we examine similar computations which have been made on the tails of other great comets we see that the two branches resemble the two types most frequently observed. The right-line tail corresponds to the principal appendages of the great comets of 1811, 1835 (Halley's), 1843, 1861, 1862, and others. The general direction also conforms to that of the secondary tail of the great comets of 1858, 1874 and others; but in the present case the light of this tail is relatively far more conspicuous. The branch of greater curvature finds its representative in the great majority of comets which have been observed.

Ommastrephes illecebrosus Verrill.

Stations 918, 919, 923–925, 939, 940, 949, 1025, 1033; 45–258 fathoms.

Taonius pavo (Les.) Steenstrup.

Station 952; 388 fathoms. Two specimens. This rare species has not been recorded from our coast, since it was described by Lesueur, in 1821.

Rossia sublevis Verrill.

Stations 924, 925, 939, 945–947, 951, 952, 997, 1025, 1026, 1028, 1029, 1032, 1033; 106–388 fathoms. Some of the specimens, recently obtained, agree more nearly with *R. glaucopis* Lov., as figured by G. O. Sars, than any seen before. It may prove to be identical.

Heteroteuthis tenera Verrill.

Stations 918, 919, 920, 921, 922, 940, 944, 949, 950, 1026, 1027; 45–182 fathoms. Eggs of this species were taken at stations 922, 940, 949, and in several localities in 1880. They are nearly round, ivory-white or pearly, attached to shells, etc., by one side, in groups, or scattered. On the upper side there is a small conical eminence.

Sepiola leucoptera Verrill.

Stations 947, 952, 998, 999, 1026 (3 juv.); 182–388 fathoms.

Octopus Bairdii Verrill.

Stations 925, 939, 945–947, 951, 952, 994, 997, 998, 1025, 1026, 1028, 1033, 1035; 103–388 fathoms.

composed of suckers, like the outer rows; but the horny parts had been destroyed, in my specimen, and the hook-shaped form of the fleshy part of the suckers was probably due to post-mortem changes. By careful treatment with reagents I have been able to restore some of the distal ones more completely, so as to show a distinctly sucker-like form.

It would, however, be difficult, without farther evidence, to believe that *Gonatus amœnus*, as figured by G. O. Sars, is the young of this species, for he neither mentions nor figures the remarkable series of lateral connective suckers and tubercles on the tentacular clubs, though he gives detailed figures of the club and its other hooks and suckers. That so careful an observer as Sars should have overlooked such a structure seems almost incredible. The two small specimens that I have hitherto seen from America, agreed well with Sars' figures, but both were considerably injured from having been in fish-stomachs. A small specimen (mantle 30^{mm} long) recently taken by us, at station 1031, is, however, well preserved, and while agreeing with *G. amœnus* in all other respects, it has the peculiar lateral connective suckers and tubercles of the club, seen in *G. Fabricii*, adult. These organs are, however, very minute in this specimen, but sufficiently evident to convince me that Steenstrup is correct in considering *G. amœnus* the young of *G. Fabricii*.

Since Steenstrup has shown that the type of my genus *Lestoteuthis* is the same genus as *Gonatus* (adult), and therefore that *L. robustus* (Dall), doubtfully referred to it by me, is a distinct genus, I propose to make the latter the type of a new genus: *Moroteuthis*. Its most prominent distinctive character will be the remarkable solid cartilaginous cone, superadded to the end of the pen, and corresponding in form and position with the solid cone of *Belemnites*.

Alloposus mollis Verrill.

Stations 937, 938, 952, 953, 994; 310–715 fathoms. Two very large females were taken: one at station 937, in 506 fathoms; the other at 994, in 368 fathoms. The former weighed over 20 pounds. Length from end of body to tip of 1st pair of arms, 31 inches; of 2d pair, 32; of 3d pair, 28; of 4th pair, 28; length of mantle beneath, 7; beak to end of 4th pair of arms, 22; breadth of body, 8·5; breadth of head, 11; diameter of eye, 2·5; of largest suckers, ·38.

The only additional Pteropod taken this year is *Triptera columnella* (Rang), from station 947. Among the Gastropods there are a considerable number of species not obtained last year. Perhaps the most remarkable discovery, in this group, is a fine typical species of *Dolium* (*D. Bairdii*) taken alive, in 202 fathoms. This genus is almost exclusively tropical in its distribution. On our coast, *D. galea* extends northward to North Carolina. This southern form, with a large *Marginella*, taken both this year (station 949) and last, an *Avicula*, and various other genera, more commonly found in southern waters, are curiously associated, in this region, with genera and species which have hitherto been regarded as exclusively northern or even arctic, many of them having been first discovered in the waters of Greenland, Spitzbergen, northern Norway, Jan Mayen Land, etc.

Among the northern species which had not been found previously south of Cape Cod, the following were dredged: *Trophon clathratus*, 972, 976; *Acirsa costulata* (=*borealis*), 965; *Amauropsis Islandica* (=*helicoides*); *Margarita cinerea*, 981; *Machœroplax bella*, 1032; *Cylichna Gouldii*, 973; *Odostomia* (*Menestho*) *striatula*, 980.

Dolium Bairdii Verrill and Smith, sp. nov.

A moderately large species, having nearly the form of *D. perdix* and *D. zonatum*. Male. Shell broad ovate, with seven broadly rounded whorls; spire elevated, apex acute; nuclear whorls about three, smooth; suture impressed, but not deep, nor channelled, the last whorl is somewhat flattened (perhaps abnormally) below the suture, for some distance, corresponding to an inward flexure of the outer lip. Aperture elongated, irregularly ovate; outer lip regularly rounded, except for a short distance posteriorly, where it is slightly incurved, its edge is excurved, acute externally, distinctly but not prominently crenulated within, except posteriorly, where a posterior canal is slightly indicated; columella straight; canal short and broad. The sculpture is peculiar: it consists of numerous (about 40 on the last whorl) rather prominent, squarish, clearly defined revolving ribs, less than 1^{mm} broad, separated by interspaces of about

the same breadth, in which there is usually one small narrow rib, alternating with the larger ones; sometimes there are two or more small ones. The whole surface, both of ribs and interspaces, is covered with fine and regular transverse, raised lines. The surface is covered with a very thin pale olive-yellow epidermis, easily deciduous when dry. Color white, except that the larger ribs are alternately light brown and white, and the apex, consisting of about three smooth nuclear whorls, is dark brown. Length 68^{mm}; breadth 56^{mm}; length of aperture 53^{mm}.

The animal is well preserved. Proboscis blackish, exserted about 20^{mm}, thick (8^{mm}) and clavate at the end, which is surrounded by a sort of collar, with a finely wrinkled or crenulated, white edge. Head large, with a prominent rounded lobe in front. Tentacles large, elongated (10^{mm}), stout, tapering, obtuse. Eyes small, black, on distinct, but slightly raised tubercles at the outer base of the tentacles. Head, tentacles and siphon-tube dull brown. Penis very large (50^{mm} long, 12^{mm} broad), twisted and thickened at base, flattened distally, terminating in a slightly prominent obtuse lobe at the tip; a well-marked groove runs along the posterior edge to the tip.

Off Martha's Vineyard, station 945; 202 fathoms. Station 1036; 94 fathoms; one young specimen and large fragments.

Pleurotoma (*Bela*) *limacina* Dall. (*Daphnella ?*)

Bulletin Mus. Comp. Zool., ix, p. 55, 1881.

Four living specimens of this elegant shell were taken at station 994; 368 fathoms. Gulf of Mexico, 447–805 fathoms (Dall). This is not a true *Bela*, for it has no operculum; eyes minute.

Capulus hungaricus (Linné).

Two living specimens were obtained, which appear to belong to this species. They are more delicate and have somewhat finer and more regular radiating ribs than the ordinary European form. It has not been recorded before from our coast.

Stations 922, 1029; 69 and 458 fathoms.

Fiona nobilis Alder and Han.

British Nud. Moll., Æolidæ, Fam. 3, pl. 38 A.

A large and handsome *Fiona*, apparently this species, was found in two instances, in large numbers, on pieces of floating timber, among Anatifers, at stations 935 and 995. They were kept in confinement several days and laid numerous clusters of eggs. These are in the form of a broad ribbon, spirally coiled in about one and a half turns, so to form a bell-shaped or cup-shaped form, and attached by a slender pedicel, so as to hang from the under sides of objects. Alder and Hancock recorded its occurrence, in a single instance, at Falmouth, England.

Issa ramosa Verrill and Emerton, sp. nov.

Body elevated, convex above, elongated, oblong, sides nearly parallel along the middle; foot well-developed, as broad as the body. Dorsal tentacles thick, clavate, obtuse, with numerous lamellæ; sheath scarcely raised. Back and sides with numerous small, simple papillæ. Along the lateral margins of the back there is a carina, with a row of large, much branched papillæ, alternating with much smaller ones; of the large ones there are about six on each side, the most anterior are below the dorsal tentacles; two on each side are posterior to the gills, the last ones largest; a row of similar but smaller processes extends below the tentacles and around the front margin.

Gills five, arborescently branched. Color, pale yellow. The dorsal tentacles darker.

The radula is quite different from that of *I. lacera* and *Triopa claviger*. The median area is wide, with two rows of thin, transversely oblong plates; there are three rows of large, nearly equal teeth on each side, with the tips strongly incurved, obtuse; the innermost tooth has a small lobe on the middle of the inner edge; these are followed by about seventeen or eighteen smaller, oblong plates, with slightly emarginate anterior ends; these gradually decrease in size toward the margins of the radula.

Stations 940, 949; 130 and 100 fathoms.

In form, this resembles *I. lacera*, but is easily distinguished by the branched appendages along the sides.

Of the Lamellibranchiata, some very interesting new forms occurred. The most important of these are species of *Pholadomya*, *Mytilimeria* and *Diplodonta*,—three genera not before found on this coast. The *Pholadomya* is more related to certain fossil forms than to any of the few described living species. The genus *Mytilimeria* has hitherto had very few living representatives, and none of them resemble our very singular species.

Among the northern forms, not previously found south of Cape Cod, are the following: *Mya truncata; Spisula ovalis* (975, 976, 981); *Leda tenuisulcata* (973); *Nucula tenuis.*

Pholadomya arata Verrill and Smith, sp. nov.

Shell triangular, short, wedge-shaped, posterior end angular, somewhat produced, obtuse; anterior end very short and abruptly truncated, clearly defined by a carina extending from the beak to the outer margin; anterior to the carina there is a broad concave furrow, which bounds the slightly convex central area of the front end; the greater part of the sides of the shell is covered with deep, rather wide, concave furrows, separated by elevated, sharp-edged ribs; the furrows vary in width and decrease

posteriorly; a small portion, near the tip of the posterior end is covered only by slight ribs. The surface between the ribs is finely granulated. When the thin superficial layer is removed the surface is pearly. The umbos are prominent, strongly incurved, nearly or quite in contact. The hinge in the right valve consists of a small, slightly prominent lamella, running back as a low ridge, and separated from the margin of the shell anteriorly, and from the cartilage-lamina posteriorly, by a narrow groove; the cartilage-pit is long, running forward under the beak as a a narrow furrow; it is bounded internally by a prominent lamella. Length, 36^{mm}; height, 29^{mm}; breadth, 26^{mm}.

Stations 940, 949, 950: 69 to 130 fathoms.

Three specimens, all dead, but one is very fresh.

Mytilimeria flexuosa Verrill and Smith, sp. nov.

Shell obliquely cordate, short, higher than long, very swollen, the anterior end rather shorter than the posterior; umbos very prominent, beaks much incurved, pointed and turned forward, with a small, deep concavity just under and in front of them. The outline and surface of the shell is very flexuous, owing to the broad deep grooves and elevated ribs which divide the surface into several areas. The most prominent rib is very high and rounded, and runs from the beak to the extreme ventral margin, inclining somewhat forward; in front of this the anterior area is flattened with a wide shallow concave groove or undulation in the middle, and others less marked; the front edge is broadly rounded, slightly undulated below. The middle area is very elevated, and forms more than a third of the shell; it is flattened or slightly concave in the middle, and undulated by several faint broad ribs; it recedes posteriorly, and a broad concave furrow separates it from the small posterior area, which is without ribs, and has a prominent rounded edge. The surface is finely granulated, lines of growth evident. The interior is pearly, angulated by a deep groove, corresponding to the largest external rib. The dorsal hinge-line is nearly straight posteriorly, and strongly incurved anteriorly, in the right valve it projects inward, but not in the left; in the right valve there is a small rounded tubercle, a little back of the beak; from below this a short rib-like process runs back below the deep, partially internal cartilage-pit, which extends forward and upward under the beak as a narrow furrow. Anterior muscular scar deep; posterior one larger ovate, less distinct; sinus small. Length, 25^{mm}; height, 26^{mm}; breadth from side to side, 22^{mm}.

Station 947; 312 fathoms. One pair of fresh valves, dead.

This and the preceding were both taken by means of the "rake-dredge."

Diplodonta turgida Verrill and Smith, sp. nov.

Shell large for the genus, round-ovate, a little longer than high, very swollen; the two ends nearly equally rounded, the anterior a little narrower; ventral edge broadly and regularly rounded; beaks nearly central, somewhat forward of the middle, strongly curved inward and forward, acute. Surface without sculpture, smooth except for the evident lines of growth. In the right valve there are, opposite the beak, two nearly equal, stout, sharp teeth, separated by a space of about the same width; back of these, and partly joined at base to the posterior one, there is a much larger, broad, stout, obtuse tooth, with a groove on its dorsal side; external cartilage-groove and its lamella are long and narrow, curved. Length, 29^{mm}; height (umbos to ventral edge), 25^{mm}; breadth, 23^{mm}.

Station 950; 69 fathoms. One right valve.

ART. XLII.—*Note on the Tail of Comet b*, 1881; by LEWIS BOSS. With Plates V and VI.

THE changes which took place in the aspect of the tail of the great comet of 1881, during the last days of June, seemed to me of peculiar and unusual interest. Appearances so novel and unexpected moved me to prepare some rude sketches of the tail, with brief notes as to its position in the sky. From several causes my opportunities for making such studies proved to be very few, and lack of experience contributed to diminish the completeness and accuracy of the results actually obtained. It is to be regretted that the number of those who give serious and systematic attention to this branch of observation is quite small in view of the small number of opportunities; while, on the other hand, the observations which can be made are uncertain in character, and the results vary much with individual judgment. It is therefore important that drawings and descriptions should be gathered from as many sources as possible.

The engravings (Plate V), accompanying this paper were reduced from drawings compiled from the original sketches and notes.

These were made in the open air at the times of observation indicated. In all cases the chief object of interest was what may be conveniently termed the right-line tail, which was far more conspicuous than the other branch on June 26, scarcely perceptible on June 28, and entirely wanting on July 1. It is to be regretted that on these dates charts were not used in the preparation of the original sketches, except for reference. The final drawings were laid down on copies of Schwinck's polar

chart (1850) from the original sketches and notes. On July 22 the outlines of the tail were drawn with care on the *Durchmusterung* polar chart (Argelander, 1855), and from thence accurately transferred to the finished sketch. The distortion of figure, owing to the projection used, is not important in any case, and for the purposes of this communication it is inappreciable. The engraver has been very successful in preserving the accuracy of the original drawings, and in imparting to them the desired effects. The following is the substance of the notes recorded:

June 26, 10^h.—Air wonderfully transparent. The tail of the great comet consists of two branches. The principal branch appears to be perfectly straight, and passes about two degrees to the apparent east of Polaris and eight or ten degrees beyond it. For the last ten or fifteen degrees this branch is exceedingly faint. The other is curved quite strongly to the apparent west, and after its separation from the principal ray requires most careful scrutiny for its detection. It seems to extend to a point six or seven degrees, astronomically southeast from Polaris.

June 26, 13^h 30^m. Sketch.—The tail presents to the naked eye much the same appearance as it did earlier in the evening, except that neither branch can be traced so far as then seen. The straight branch appears to pass quite centrally over 2 Ursæ Minoris, and to extend about two degrees beyond B. A. C. 7851. Its breadth seems to be nearly uniform and a little more than one degree. With the aid of a straight edge no curvature could be safely assigned. There is a rather sudden falling off in brightness at a point four or five degrees from 2 Ursæ Minoris toward the nucleus. The edges of this ray are ill-defined and the central parts brightest. The ray which curves toward greater right ascension is not satisfactorily seen. Its effect is to broaden and intensify the principal ray for a distance from the nucleus equal to about four-tenths the whole distance to Polaris. At this point the total breadth of the tail is estimated to be about four degrees. Here a separation is faintly indicated, but the continuation of the curved ray is observed with extreme difficulty. The direction and extent of this branch is indicated on the sketch.

June 28, 13^h. Sketch.—Foggy haze low down in the north. Sky otherwise satisfactory. The nearly straight ray described on June 26 has dwindled to a faint and narrow streak, which might have been overlooked, had not a bright one been expected in its place. It extends to a point near 2 Ursæ Minoris as indicated in the sketch. Its breadth is not over one-third of a degree. The curved branch is brightest in its central parts, and is very conspicuous for the first ten or fifteen degrees of its length. It seems to terminate about three degrees short of B. A. C. 4349; though at times a much greater extent is suspected. Fifth magnitude star (B. A. C. 2326) is 15′ inside the following edge of the tail. The axis of this branch passes to the apparent east of

B. A. C. 4349, and at a distance from it equal to about one-fifth the distance between that star and Polaris. The last direction of the axis is toward β Ursæ Minoris. The distance of Polaris from the preceding edge of the tail is nearly equal to the distance between Polaris and 2 Ursæ Minoris. The breadth at three-fourths the distance from the nucleus is about three degrees.

July 1, 12^h 15^m. Sketch.—State of sky not remarkably fine. The tail is much shorter than heretofore, and its appearance entirely changed. There is no trace of the straight ray seen on June 26 and 28. The preceding edge of the tail appears nearly straight. It is brighter and extends to a greater distance from the nucleus than the following edge. The latter is strongly curved near the end. The breadth is about three degrees at the widest part.

July 13, 10^h 15^m.—Tail single, faint, and diffuse. Estimated length seven degrees. Breadth near the end, about 40′. The direction of the axis prolonged passes to the east of ε Ursæ Minoris, at a distance about one fifth that between ε and δ Ursæ Minoris.

July 22, 14^h. Sketch.—Four-inch Clark Comet seeker. Power twelve. Field 2° 30′. Sky fine. Two branches seen. The first is nearly straight and brighter than the other. Estimated width 10′. This branch is certainly recognized as far as A. R. 14^h 20^m. Sometimes I imagine that it extends as far as A. R. 15^h 40^m. [As indicated by the dotted line in diagram.] The light seems to be composed of a great number of parallel bright streaks. This appearance of striation is very decided in the region within two degrees of the nucleus. The southern branch is curved and much shorter and fainter than the straight ray. The location of the last degree of length represented in the sketch is very difficult. The breadth here is estimated to be 30′ or 40′. The bounding lines are carefully laid in on the *Durchmusterung* chart, and their position relatively to stars frequently compared with the sky during the progress of the sketch. Sky suddenly clouded at 14^h 30^m.

During the remainder of July the appearance of the tail did not essentially change. I was absent from the observatory for a short time in the early part of August, and did not again obtain a telescopic view of the tail until August 17. It was then apparently single. The estimated length was 3°. There are slight inconsistencies in the notes of June 28, which have been adjusted according to the supposed weights of the various estimations.

For the points most carefully determined, and with such approximation as appears to be warranted by the precision of the observations, we have for positions of points in the tails on the respective dates:

TABLE I.

	Nucleus.		Axis, right-line tail.		Curved tail.		Point in curved tail observed.
	a	δ	a	δ	a	δ	
June 26, 13^h 30^m	87°·2	57°·9	316°	83°·0	99°	80°·5	Axis.
	----	----	13·2	85·6	----	----	
June 28, 13^h 00	90·1	63·9	20·	85·9	155	86·0	Axis.
	----	----	----	----	100	83·0	Axis.
July 1, 12^h 15^m	95·8	70·7	----	----	111·2	87·0	Prec. edge.
	----	----	----	----	115·3	80·0	Foll. edge.
July 22, 14^h 00	177·6	81·9	215·4	82·8	205·0	82·2	Axis.

It would have been better, no doubt, to have made no special effort to determine the position of the extreme visible limit of the tail, but to have given greater attention to the position of the axis and the breadth of the visible portions at points where the tail could be easily seen. But even with the present imperfect data, we shall be able to derive some idea of the real position of the tails in space, and of their correspondence in type with others which have been observed.

Convenient formulæ have been devised by Bessel (Astr. Nachr., vol. xiii, p. 193), by the use of which we may determine the angular deviation of a point in the tail from the radius vector prolonged. It will be necessary to assume that the axis of the tail lies in the plane of the orbit of the nucleus. This assumption is well supported both by theory and experience, and is, no doubt, substantially correct. Such small deviations as might result when emissions of matter from the head are unsymmetrical with reference to the orbit plane, or when the initial velocity of particles thrown off from the nucleus is greater toward one pole of the orbit than toward the other, may probably be neglected as comparatively insignificant. Let:

r=Radius vector of the nucleus at the time of observation.
ρ=Geocentric distance of nucleus.
Δ=Length of tail, or distance of point observed from the nucleus.
s=Angular length of tail.
p°=Position angle at the nucleus of r prolonged.
p=Corresponding angle for the observed point in the tail.
S=The cometocentric distance of the earth from the north pole of the comet's orbit.
T=Cometocentric angle between the earth and the observed point in the tail.
φ'=The cometocentric angle between the observed point and the radius vector prolonged,—positive, when this point is on that side of the radius vector from which the comet has been moving.

From the elements of Dr. Oppenheim (Astr. N., 2384), we find for the coördinates of the north pole of the orbit of comet b, referred to the equator,

$$A=192^\circ\ 09'. \qquad D= +23^\circ\ 46'.$$

We then derive the following table of results:

TABLE II.

	June 26.			June 28.			July 1.		July 22.	
	Axis of right-line tail at end.	Axis of right-line tail at 2 Urs. Min.	Axis of curved tail at end.	Axis of right-line tail at end.	Axis of curved tail at end.	Axis of curved tail near B. A. C. 2326.	Preceding edge at end.	Following edge at end.	Axis of right-line tail at end.	Axis of curved tail at end.
r	·763	----	----	·775	----	----	·795	----	1·018	----
ρ	·340	----	----	·374	----	----	·428	----	·853	----
Δ	·210	·187	·179	·161	·189	·153	·130	·110	0·82	·057
s	37°·0	31°·1	22°·9	25°·0	24°·7	19°·2	16°·4	10°·4	5°·0	3°·9
p°	345·6	----	----	348·6	----	----	353·1	----	57·7	----
p	351·2	351·8	5·0	350 8	8·7	3·7	2·8	18·6	61·5	71·8
S	102·9	----	----	106·1	----	----	110·3	----	121·6	----
T	40·2	39·1	24·9	54·0	30·0	34·4	51·8	32·9	109·8	94·1
ϕ'	12·9	14·0	29 8	5·6	32·1	27·0	21·1	41·5	6·2	24·9
							$\underbrace{\qquad\qquad}_{31·3}$			

An inspection of the foregoing table shows that the characteristics of the two branches of the tail, as defined by the values of φ', present a similarity quite as striking as could have been predicted in view of the considerable probable errors to which such determinations are liable. On the first three dates the cometocentric elevation of the earth above the plane of the comet's orbit was, respectively, 13°, 16°, and 20° only; so that small errors in the observed position angle are considerably multiplied when converted into the corresponding values of φ'. It must also be remembered that many of the points observed are several degrees farther from the nucleus than the superior limit of visibility assigned by most observers for the extent of the tail on the respective dates.

So far as I am aware most of the observers who have already reported on the appearance of the tail failed to notice the division into branches at all. On the other hand, it cannot be supposed that this interesting aspect entirely escaped detection under proper conditions of sky and terrestrial surroundings.

If we examine similar computations which have been made on the tails of other great comets we see that the two branches resemble the two types most frequently observed. The right-line tail corresponds to the principal appendages of the great comets of 1811, 1835 (Halley's), 1843, 1861, 1862, and others. The general direction also conforms to that of the secondary tail of the great comets of 1858, 1874 and others; but in the present case the light of this tail is relatively far more conspicuous. The branch of greater curvature finds its representative in the great majority of comets which have been observed.

The tail of the comet of 1807 presents most striking resemblance to this under discussion. On October 22, 1807, the comet of that year had, generally speaking, the same position in space as the present comet had on July 22. On that occasion (Astr. Nachr., vol. xiii, p. 228), Bessel found two tails. The first he considered to be nearly straight and in length about 4·5°. The other was strongly curved, broader than the first, and in length about 3°. Dr. Bredichin (Mosc. Ann., vol. v, pt. 2, p. 56), has computed the value of φ' for the end of each tail. This enables us to compare the two descriptions in a very satisfactory manner. We have—

	Comet of 1807.			Comet of 1881.		
	Δ	ρ'	s	Δ	ρ'	s
For the right-line tail, - -	·139	7°·9	4°·5	·082	6°·2	5°·0
For the curved tail, - - -	·105	24·2	3·0	·057	24·9	3·9

Allowing for the difference in values of Δ and r, the agreement is quite within the probable errors of observation. It is thus seen that there is great similarity in the physical appearance of the two comets, as well as between the elements of their respective orbits. Since, in general, we have the greatest possible variety in the appearance of the tails of the comets, and especially in the combination of tails of different types, we may confidently say, that the very remarkable similarity above shown furnishes another important fact, in addition to those which already tend to indicate a common origin for the comets of 1807 and 1881.

Sir Isaac Newton and others after him have shown that the tail might be produced by a repulsive force emanating from the sun, and acting on detached particles, which are continually thrown out from the nucleus of all great comets. Bessel has investigated formulæ (Astr. Nachr., vol. xiii) which enabled him to compute the repulsive force necessary to produce a tail of the form actually observed in the case of Halley's comet. The repulsive force in these formulæ is, of course, an implicit function. Bessel's formulæ are shown (Mosc. Ann., vol. v, pt. 2) to give results which are but roughly approximate for large distances from the nucleus. Professor Norton, Dr. Bredichin and others have published formulæ which are more rigorously exact. In all these investigations it is supposed that a particle projected from the nucleus is repelled by a force $(1-\mu)$ the reverse of the Newtonian. The effective force acting on the particle will be μ, and when combined with the tangential velocity of the nucleus will cause it to describe a hyperbolic orbit. This hyperbola will be convex or concave to the sun, according as $(1-\mu)$ is greater or less than unity. In the volumes of the Moscow Annals, Dr. Bredichin presents a variety of reasearches concerning the consequences to be deduced from this assumption of repelling forces.

He refers the tails of comets to three general types, distinguished by the value of $(1-\mu)$ employed in their theoretical representation. The value of $(1-\mu)$ (expressed in the Newtonian unit) for Type I is 11·0 to 12·0; for Type II, about 1·3; for Type III, 0·3, or less. The value of $(1-\mu)$ for Type II, however, is found to vary considerably in different cases without losing its distinctive character. It is possible to introduce the effect due to the initial velocity of projection from the nucleus, and this, of course, modifies the value of $(1-\mu)$ which would otherwise be assumed. This effect will evidently be proportionally least in tails of Type I, and will increase in importance as the value of $(1-\mu)$ is diminished. If we suppose particles to be projected from the nucleus equally in all directions with equal velocities, the effect will be mainly shown in the breadth of the tail. Thus we invariably find tails of Type I to be narrow in comparison with those of Type II,—a fact which finds satisfactory explanation in the relatively small effect, which would be produced by the action of initial velocity, when the repelling force is relatively very great. But since cometary emissions appear to take place mostly on the side of the nucleus nearest the sun, the assumption of the value zero for initial velocity will always render the value of $(1-\mu)$ computed from observation, too small.

It will be interesting to examine our observations of the tail of comet *b* 1881, with a view to determining to what extent they conform to the normal types. In a preliminary discussion like this, which is founded on few observations of small weight, it will not be worth while to include the effect of initial velocity of emission. When a great number of observations of the tail and coma have been collected, it may be possible to arrive at some satisfactory result in this direction. I have accordingly computed the hyperbolic orbits of particles emitted from the nucleus at various times (previous to the observations on the tail), with values of $(1-\mu)$ equal to ·6, 1·0, 1·4 and 11·0. The values of the radius vector and true parabolic anomaly of the nucleus have been computed from the elements of Dr. Oppenheim, previously cited.

Let:

M = Date when a given particle is observed in the tail.
M′ = Time of emission of that particle from the nucleus.
M″ = Perihelion passage of the particle.
E = Eccentricity of the hyperbolic orbit.
I = Angle between the radii vectores of the particle and nucleus at the time M. For the particle referred to the nucleus, this angle will evidently always be retrograde to the motion of the nucleus.
Δ = Distance of the particle from the nucleus at the time, M.

η = Length of perpendicular let fall from the particle on r produced at the time, M.

ξ = Distance from the foot of that perpendicular to the nucleus.

φ = Angle whose sine is $\frac{\eta}{\Delta}$, or the angle between r prolonged and the line joining the nucleus and particle at the time M.

As an example of the manner in which the theoretical lines of Plate VI have been constructed, the results of computations intended to represent the right-line tail of June 26·805 (Berlin time) are subjoined. The value of $(1-\mu)$ is assumed to be 11·0; and the hyperbolical orbits are computed for particles emitted at perihelion, and for two designated dates subsequent to that time. We have:

M′	June 16·510	June 18·510	June 20·510
M″	June 16·510	June 18·359	June 20·205
log E	0·0792	0·0791	0·0788
I	4° 15′	2° 29′	1° 13′
Δ	·284	·192	·110
ξ	·273	·187	·109
η	·077	·041	·018
φ	15°·7	12°·4	9°·5

From the values of Δ, ξ, and η, the curve marked I in the figure for June 26 (Pl. VI) is constructed. From that curve we derive by a graphic process the values of φ corresponding to the observed values of Δ at two points in the tail on that date. We thus have:

	Δ	ϕ'	ϕ
June 26·805	·210	12°·9	13°·2
Type I	·187	14·0	12·3

The agreement between the values of φ' and φ is even closer than could have reasonably been expected from the unavoidable probable error in the determination of φ'.

In the diagrams of Plate VI, the point N represents the position of the nucleus at the respective times of observation. N R′ is the radius vector prolonged. The curves N I are carefully constructed in the original diagrams from the computed positions of two or more particles, when $(1-\mu)=11·0$. The previous dates of emission were so chosen that one or more computed points would fall near that which was actually observed. The curves N II were constructed with $(1-\mu)=1·4$, and may represent the tail of Type II. The intervals between dates of emission and observation for like values of Δ are much greater in this case than in that for tails of Type I. The curves N II″ are constructed with $(1-\mu)=1·0$; and N III″ for July 22, is based on $(1-\mu)=0·6$. The dots enclosed in small circles indicate the positions of points in the tail actually observed.

The computed positions of these are given in table II. The dotted lines are intended to give a rough idea of the outlines of the tail as observed and reduced to the plane of the orbit, on the somewhat doubtful assumption that the thickness of the tail may be neglected in comparison with its breadth in the plane of the orbit. Following is a tabular view of the results obtained by computation, with the corresponding values from observation.

TABLE III.

Date.	Type I. $(1-\mu) = 11{\cdot}0$.					Type II.					
	Point	Δ	ϕ'	ϕ	$\phi-\phi'$	Point	Δ	ϕ'	ϕ	$\phi-\phi'$	$(1-\mu)$
			°	°	°			°	°	°	
June 26	I_1'	·210	12·9	13·2	+ ·3	II′	·179	29·8	31·2	+1·4	1·4
	I_2'	·187	14·0	12·3	−1·7						
June 28	I′	·161	5·6	11·0	+5·4	II_1'	·189	32·1	32·8	+ ·7	1·4
						II_2'	·153	27·0	29·7	+2·7	1·4
July 1						II_1', II_2'	·120	31·3	26·5	−4·8	1·4
July 22	I′	·082	6·2	5·7	− ·5	II′	·057	24·9	12·	−13·	1·4
									16·	− 9·	1·0
									21·	− 4·	·6

A value of 0·4 for $(1-\mu)$ would give a fair approximation to the tail of Type II as observed on July 22. The agreement of the observed and the computed values of φ for the tail of the first type is very satisfactory. The deviation of five degrees on June 28 might easily be attributed to errors of observation on an object which was so excessively faint; and it is quite probable that the location of the end point was somewhat influenced by the general direction of the tail nearer the nucleus where it was brighter. Such an influence would tend to make the observed value of φ too small. The two values of φ' best determined for Type I are the second and fourth of the table; and these both indicate that a smaller value of $(1-\mu)$ should have been employed.

With reference to the comparisons of observed and computed φ in the tail of the second type, we do not expect an accordance so satisfactory. The difficulties of observation were greater with this branch of the tail, which was broad and faint at its extremity; and, furthermore, an error in location of this shorter branch would have a greater influence upon the value of φ'. The probable uncertainty in the value of φ' for the first three dates I estimate at three or four degrees. On July 22 the location of the shorter branch of the tail was extremely difficult; still I cannot think that the probable uncertainty in φ' is greater than four or five degrees. This would make any value of $(1-\mu)$

much greater than 0·6, extremely improbable for that date, unless we suppose a high velocity of emission from the nucleus mainly on the side nearest the sun. The particles seen near the end of this branch of the tail must have left the nucleus about July 4, and for portions nearer the head at later dates. We know that this period was one of great activity in the nucleus, and it is reasonable to suppose that the velocities of emission toward the sun were unusually great. It is worthy of remark that the value of φ' obtained from Bessel's observation of the 1807 comet (Oct. 22) requires a value for $(1-\mu)$ of about 0·6. (Mosc. Ann., vol. v, pt. 2, p. 56). We may, however, suppose that the matter composing the tail of July 22, having been exposed to a lower temperature at the time of emission than that which prevailed at perihelion, was in a less finely divided state. Then, on the theory of electrical repulsion, we should expect to find a smaller repulsive force for the later date.

On the whole, the results which can be inferred from table III in respect to the ratio of the repulsive forces concerned in the genesis of the two tails, may be regarded as extremely favorable to the hypothesis of Dr. Bredichin, viz: that the tail of Type I is due to the presence of hydrogen in the comet, and that of Type II to carbon. Granting this, we should have expected the traces of hydrogen in the spectrum of the comet to have been very pronounced on June 26, or on dates immediately preceding. On June 28 and for a few days following that date, we might look for a weakening of the hydrogen lines, or, at least, a decided change in the character of that portion of the spectrum. It must be confessed, however, that all reasoning in the premises must necessarily be vague and unsatisfactory, since we do not know to what extent matter, in the state in which it must exist to form the tail, contributes to the spectrum of those parts of the comet in the vicinity of the nucleus and coma, where, alone, spectra have been successfully observed.

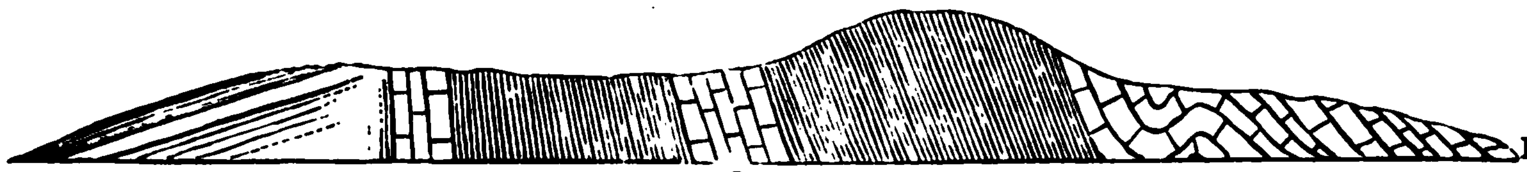
The complete history of this comet, of the changes observed in the nucleus and its surroundings and in the tail, with drawings, measures and estimated dimensions of all parts will be extremely interesting. When collected and combined with results of polariscopic and spectrum analysis, it will doubtless furnish most valuable material bearing upon the true theory of the constitution of comets. That such material exists in rare abundance it is not permitted us to doubt; and it is to be hoped that no one who is in possession of definite results, however meager in quantity, will hesitate to add them to the collection.

Dudley Observatory, September 8, 1881.

ART. XLIII.—*Geological Relations of the Limestone Belts of Westchester County, New York;* by JAMES D. DANA.

1. *Section of the Mott Haven belt of Limestone on* 122*d St., New York Island.*

As the outcrops of limestone on New York Island will soon be graded away, I here supplement my notice of the 122d street locality with a fuller account of the section there afforded, and a figure representing it. The section is on the north side of this street, and extends from Lexington avenue to the first dwelling house—about 120 feet. There are three belts of limestone: *a*, five feet wide; *b*, seven feet; and *c*, in view to the eastern limit of the open lot, 32 feet. The band *g* looks, at first, as if it were the westward dipping portion of *a*, but it is in reality a seam of granite or granitoid gneiss, in the schist. The three limestone masses *a*, *b*, *c*, appear here to be independent beds. But over the

W.  E.

g *a* *b* *c*

open lot, thirty yards north of *a*, *b*, the limestone *b* widens *eastward*, or toward *a*, to 25 feet, and only a thin layer of schist separates it from the continuation of *a;* moreover the beds dip eastward under the schist at an angle of only 45°. Forty feet farther north, in the back yard of a house fronting on 123d street (the next north), the western of the bands of limestone widens in both directions, and, from a high westward dip on the west side, bends over eastward to horizontality. From these facts it is probable that *a* and *b* are the two sides of an anticlinal; that this anticlinal has its axis dipping northward, so that the intervening and therefore *underlying* schist disappears to the northward, while the limestone stratum becomes broadly exposed between the overlying schist on the east and west. Some of the schist on the corner of Lexington avenue and 123d street I found to be fibrolitic. On 123d street, only schist is in sight; the middle portion of the area, or that in the direction of the axis of the anticlinal, is occupied by houses. Whether the limestone *c* is the same stratum, brought up by a fault or flexure, or, as it seems to be from its position and thickness, another, I cannot say. If overlying, its continuation would naturally be looked for to the westward, where it is not known to occur. The schist is much rusted and its bedding poorly exposed: moreover, the outcrops of limestone south of 122d street differ widely from those on its north side; and for these reasons it is difficult to reach any positive stratigraphical conclusions.

This locality is on the western border of the Mott Haven limestone belt. As to the eastern border, nothing is here indicated, beyond this, that the limestone increases in amount to the eastward.

Mr. Stevens's figure of the section on 122d street, in the Annals of the New York Lyceum, referred to on p. 432 of the last volume of this Journal, is so very unlike what I have found at the place, and agrees in so many points with that of the more western belt on 132d street, of which also he speaks in the article, that I have suspected it to be wrongly labelled.

2. *Contact-phenomena in the Schist and Soda-granite of Cruger's and Stony Point.*

In my remarks on the rocks at Cruger's and Stony Point I have sustained the view that the contact-phenomena, as they may in a literal sense be called, between the mica schist and soda-granite, are not results of contact of the schist with a pasty or liquid rock. I add here a few more words on this point.

The contact-phenomena are these. The mica schist changes, over the interval of about 1,000 feet between the limestone stratum on the south and the granite on the north, (1) from a nearly even-bedded condition to a much-flexed one—it becoming bent up to the northward in many places into close and deep zigzags; (2) from a finely crystalline state to a coarsely crystalline—in connection with which change there is an increase in the size and abundance of garnets; (3) from a garnetiferous mica schist, to a staurolitic and fibrolitic mica schist, with also an increasing abundance of garnets; (4) from a near freedom from quartz seams to a condition of crowded interlamination with them; and (5) occasionally, near the granite, from its ordinary micaceous and quartzose character to a feldspathic and gneissoid, in which oligoclase occurs with the orthoclase and the constitution thus approaches that of the granite. Besides the above, the granite often contains (6) scattered garnets near the junction, and also (7) both near and remote from the schist, numerous inclusions of schist, many of them short fragments, others long, flexed, or zigzag layers, parallel in position to the bedding of the schist outside, some fading nearly into the granite and vein-like, others, especially if staurolitic, having all the characters of such layers in the outside schist.

The following considerations are believed to confirm the correctness of the conclusion to which I have been led as to the origin of these contact-phenomena.

(1) The zigzag flexures of the mica schist—a rock of great firmness, rendered eminently so by its numerous quartzose interlaminations—must have been made at the time when its metamorphism took place; for their production after it was in its solid crystalline condition would be impossible, or, at least so without its having every where evidence of fracturing.

(2) The zigzag and other flexures in the schist indicate that great pressure was exerted from some direction against the stratum of slate (and other strata of the series) under conditions fitted to produce them. A yielding liquid or pasty rock, however forcibly intruded, would be a very feeble agent for such work, and would have afforded feeble resistance to pressure from other agencies.

(3) The increase in the grade of metamorphism, sufficient moisture being present, would have needed no other cause but an increase in the degree of heat; and this would have been, in any case, a consequence of the increasing extent of the flexures or of internal movements in the schist; for the constituent minerals consist only of the common ingredients of sediments, and increase in abundance of garnets means little more than increase in amount of iron.

(4) Staurolite and fibrolite are minerals that occur widely distributed through mica schists; and require for their formation, not contact-conditions, but too little alkali with the silica and alumina in the original bed-material to make a feldspar.

Whatever, then, the origin of the granite, the schist must have been put into its present flexed condition before there was any liquid or pasty rock in front of it. Further, whatever the schist has of crystallization or of crystallized minerals may have been produced independently of any such condition. Finally, the above facts, and others mentioned under the head of contact phenomena showing transitions between the two rocks, are opposed to the idea that the granite is of exotic eruptive origin, and are well explained on the view of simultaneous metamorphic changes in two adjoining conformable sedimentary formations that had some intermediate gradations and intercalations, in which the granite-made portion passed to a pasty state and so became in some places an intrusive rock.

The mica schist and the adjoining limestone are strata in a great synclinal or anticlinal fold, and probably, as I have shown, the former. But whether the fold be a synclinal or anticlinal, the increase northward observed in the zigzag flexures and in the metamorphism is *increase toward the axial plane of the fold.* The minor zigzag flexures may have been the effect either of the pressure that produced the great fold, or of the gravitation of the mass after it had been raised to a high angle—now 70°.

The facts at Stony Point are very similar to those at Cruger's. Although in some points seeming to sustain quite strongly the theory of direct eruptive origin, if viewed together with those of Montrose Point and the Verplanck Peninsula, they lead, I believe, to the same conclusion—that of a metamorphic origin alike for the soda-granite, quartz-dioryte, noryte and chrysolitic rocks. If there has ever been an example of an igneous rock made through the fusion of sedimentary beds, the cases above described may be reasonably regarded as of this mode of origin.*

For the remainder of this Appendix see the supplementary sheet at the close of this number, p. 327.

* One of the statements on page 201 of the article referred to I have to withdraw—that relating to figure 4. The figure is correct as far so it goes: but I have found, on a recent visit to the place, that the band is continued after another fault, and is not a narrower one folded on itself.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Velocity of Light.*—Lord Raleigh discusses the recent paper of Young and Forbes (Roy. Soc. Proc., May 17, 1881), in which it is maintained that blue light travels *in vacuo* about 1·8 per cent faster than red light, and asks the question: what is really determined by observations on the velocity of light? Is the velocity of a single wave determined, or that of a group of waves? If the group velocity be denoted by U and the wave velocity by V, the relation between these velocities is explained by $U = \frac{d\,(kV)}{d\,k}$, in which k is inversely proportional to the wave length. According to Young and Forbes, V varies with k and therefore U and V are different. A complete knowledge of U, which can be obtained by experiment, does not lead to a knowledge of V. Lord Rayleigh discusses the various methods employed in determining the velocity of light and concludes that if we regard the solar parallax as known, we obtain almost the same velocity of light from the eclipses of Jupiter's satellites as from observation, although the first result relates to the group velocity and the second to the wave velocity. There cannot be, therefore, a difference of two or three per cent between the group velocity and the wave velocity. These considerations lead Lord Rayleigh to doubt the conclusions of Young and Forbes.—*Nature*, Aug. 25, 1881, p. 382. J. T.

2. *Movement of Sound Waves in Organ Pipes.*—Dr. Rudolph Koenig has contrived an ingenious arrangement which enables one to observe the nodes and segments of a sound wave in its passage through an organ pipe. The pipe is slotted along its entire side, is then placed in a horizontal position with the slot beneath and resting in a trough of water. The water thus forms a portion of the lower side of the pipe and the slot allows a hollow glass tube, U-shaped, to be pushed along the interior throughout its entire length. By connecting the glass tube with manometric capsules, one can discover the position of the nodes and also observe peculiarities in the movements of the waves.—*Ann. der Physik und Chemie*, No. 8, 1881. J. T.

3. *On the Conductivity of Metals for Heat and Electricity.*—In the continuation of a paper on this subject, Herr L. Lorenz discusses the theoretical laws of the cooling of metals when placed in ordinary air and extends his observations to the conduction of heat by metals in general. If T represents the absolute temperature, k and x the conductivity for heat and electricity respectively, he is led to the following expression: $\frac{k}{x} = T \times$ constant. According to his view there is discontinuity in the interior of every body and there are regions or sections along which free electricty can move without manifesting difference of

potential or experiencing resistance. When the electricity passes through these regions, electric potential is observed. The heat state and the electrical state are interconvertible forms of energy, manifested according to the state of the body.—*Ann. der Physik und Chemie*, No. 8, 1881, p. 582. J. T.

4. *Microphonic action of Selenium cells.*—Dr. JAMES MOSER, led by the theory that one and the same ray of light may have heating, chemical and luminous effects, has examined the behavior of selenium under the influence of the electrical current. It was found that ordinary electrical polarization was manifested by selenium: with a cell composed of zinc, selenium and copper, a polarization of about 0·4 volt. was observed and a current was obtained long after it was separated from the primary battery. A careful examination of the connections between selenium and copper in the form of cells invented by Bell and Tainter, and modified by others, showed that between the copper and the selenium there is only a slight and imperfect contact. Moser therefore concludes that the selenium photophone is a microphone, and is confirmed in this belief by the action of the carbon photophone constructed by Bell and Tainter, which consists of a zigzag line scratched on a silver covered glass plate and covered with lamp-black. This instrument acts like the thermoscope described by Mr. Hughes. The illuminating rays of light are those which are especially absorbed by selenium, "only the absorbed rays can produce changes of volume and of shape and in this way influence the contact of current-conducting parts." Selenium, therefore, is heated by light and this heating effect makes the selenium cell act microphonically. The light may also produce certain chemical effects in the interior of the selenium, which may contribute to the efficiency of the cell. It was found that the resistance of certain pieces of selenium increased instead of decreased when submitted to light. Dr. Moser therefore sees no reason for separating selenium from other bodies and "no prospect of finding an unknown power or a new relation of forces in this substance."—*Phil. Mag.*, Sept., 1881, p. 212. J. T.

5. *On the stresses caused in the Interior of the Earth by the Weight of Continents and Mountains;* by G. H. DARWIN, F.R.S. —In this paper I have considered the subject of the solidity and strength of the materials of which the earth is formed from a point of view from which it does not seem to have been hitherto discussed.

The first part of the paper is entirely devoted to a mathematical investigation, based upon Sir William Thomson's well-known paper on the rigidity of the earth.* The second part consists of a summary and discussion of the preceding work.

The existence of dry land proves that the earth's surface is not a figure of equilibrium appropriate for the diurnal rotation. Hence the interior of the earth must be in a state of stress, and as the land does not sink in, nor the sea-bed rise up, the materials

* "Thomson and Tait's Nat. Phil.," § 834, or "Phil. Trans.," 1863, p. 573.

33 tons per square inch, and it would rupture if made of any material excepting the finest steel.

The stresses produced by harmonic inequalities of high orders are next considered. This is in effect the case of a series of parallel mountains and valleys, corrugating a mean level surface with an infinite series of parallel ridges and furrows.

It is found that the stress-difference depends only on the depth below the mean surface, and is independent of the position of the point considered with regard to ridge and furrow.

Numerical calculation shows that if we take a series of mountains, whose crests are 4000 meters, or about 13000 feet, above the intermediate valley bottoms, formed of rock of specific gravity 2·8, then the maximum stress-difference is 2·6 tons per square inch (about the tenacity of cast tin); also if the mountain chains are 314 miles apart, the maximum stress-difference is reached at 50 miles below the mean surface.

The solution shows that the stress-difference is *nil* at the surface. It is, however, only an approximate solution, for it will not give the stresses actually in the mountain masses, but it gives correct results at some three or four miles below the mean surface.

The cases of the harmonics of the 4th, 6th, 8th, 10th, and 12th orders are then considered; and it is shown that, if we suppose them to exist on a sphere of the mean density and dimensions of the earth, and that the height of the elevation at the equator is in each case 1500 meters above the mean level of the sphere, then in each case the maximum stress-difference is about 4 tons per square inch. This maximum is reached in the case of the 4th harmonic at 1150 miles, and for the 12th at 350 miles, from the earth's surface.

In the second part of the paper it is shown that the great terrestrial inequalities, such as Africa, the Atlantic Ocean, and America, are represented by an harmonic of the 4th order; and that, having regard to the mean density of the earth being about twice that of superficial rocks, the height of the elevation is to be taken as about 1500 meters.

Four tons per square inch is the crushing stress-difference of the average granite, and accordingly it is concluded that at 1000 miles from the earth's surface the materials of the earth must be at least as strong as granite. A very closely analogous result is also found from the discussion of the case in which the continent has not the regular wavy character of the zonal harmonics, but consists of an equatorial elevation with the rest of the spheroid approximately spherical.

From this we may draw the conclusion, that either the materials of the earth have about the strength of granite at 1000 miles from the surface, or they have a much greater strength nearer to the surface.

This investigation must be regarded as confirmatory of Sir William Thomson's view, that the earth is solid nearly throughout its whole mass. According to this view, the lava which issues

from the volcanoes arises from the melting of solid rock, existing at a very high temperature, at points where there is a diminution of pressure, or else from comparatively small vesicles of rock in a molten condition.—*Proc. Roy. Soc.*, June, 1881.

6. *Expansion of Cast Iron while solidifying.*—M. J. B. HANNAY and ROBERT ANDERSON have a paper on this subject in the Proceedings of the Royal Society of Edinburgh for December, 1879 (p. 359). By trials in different ways, the authors reach the conclusion that "liquid cast iron expands at least 5·62 per cent of its volume on freezing."

II. GEOLOGY AND NATURAL HISTORY.

1. *Origin of the Iron Ores of the Marquette District, Lake Superior;* by M. E. WADSWORTH. (Proc. Boston Soc. Nat. Hist., xx, 470, March, 1880.)—After a few prefatory sentences, this paper presents what are regarded as objections to the view of the *metamorphic* origin of the Archæan iron ores of Marquette, and a brief mention of reasons for holding that of its *eruptive* origin. The argument for their metamorphic origin, from the fact that the ore is banded, conformably to the outside schists, with layers of red jasper, and is often schistose in the same direction, is met by the remark that this banding is strongly like the banding of some rhyolites, thus making banding a character of more importance than mineral constitution. To the argument for metamorphism from original marsh-made beds, based on the fact that the ore is in bed-like masses conformable with the bedding of the associated schists, the author says—putting his objections in the unnecessary, but with him common, form of ridicule, and shooting wide of the real point at issue—that whoever advanced this theory "probably intended it for a bit of facetiousness." "A dike passing through slate must be sedimentary because the slate is sedimentary! Do we not find rocks intruded through sedimentary ones in every position, both parallel with the stratification and oblique or perpendicular to it?" and then, with still more earnestness in his misdirected logic, "Can any geologist ever have been so ignorant of the mutual association of eruptive and sedimentary rocks as to have soberly advanced the above idea?" After discussing in this style "all the evidence which we are aware has been used to prove the sedimentary origin of the jaspilite and ore," Mr. Wadsworth uses a still more personal method, the notice of which is unnecessary.

Mr. Wadsworth, in his argument for an *eruptive* origin, which follows, does not show that the iron ore and jasper are much like eruptive rocks in mineral constitution, or give facts proving that iron sesquioxide and silica, among the most infusible of minerals, may come up side by side in a state of fusion, when ordinary ejected rocks contain the iron and silica chiefly in fusible combinations; he simply asserts, "as the prominent fact," "that wherever the contact of these rocks with the country rock could be studied, that contact was always an eruptive one," of

which he says he is especially able to judge. There is nothing else of as much importance as this, and on this point his inference is not confirmed by the writer's microscopic examination of thin slices of the jasper and adjoining ore. No detailed facts or sections, or descriptions of rock-slices, are given; the deficiency of the article in this respect is one of its remarkable features. The author, after depreciating remarks about others, mentions, in his paper, the several qualifications,—geological, lithological, petrological, etc.,—required for the model investigator of the subject; and, in contrast, the paper itself contains no geological, lithological, or petrological details.

The paper closes with a statement of the author's ideas as to scientific progress, part of which we cite, that the warning it conveys may be circulated and duly heeded: "The day seems not so far distant as might be supposed, when it will again be as necessary to challenge the statements of those holding plutonic views as it is now those holding neptunian ones. The popular belief in any subject continually oscillates between different opinions like a mighty pendulum, passing and repassing the point of truth. But, strange fatality, if it stops at this point, all is stopped, the works are dead. When truth is reached or discussion ends, stagnation ensues. Again, when the pendulum vibrates, woe be to the man who swings not with it. In all candor we ask geologists to stop and think if the pendulum has not swung decidedly out of the perpendicular on the sedimentary side? Ease up a little, brethren, but do not swing back too far."—A head not out of the perpendicular is plainly very desirable. J. D. D.

2. *The Taconic rocks of the border of Lake Champlain.*—Mr. JULES MARCOU has an important paper in the Bulletin of the Geological Society of France for Nov. 8, 1880 (III, ix, No. 1, 1881), on the rocks of the northeastern border of Lake Champlain, referred by Emmons to the Taconic System, and especially upon what he regards as "colonies" in these rocks, using the term nearly as done by Barrande, for "centres d'apparition d'êtres précurseurs et de types prophétiques." These Taconic slates have in part been referred, since Barrande's article on the fossils, to the Primordial or Cambrian. The paper gives in detail the results of Mr. Marcou's study of the beds near Georgia, St. Albans, Swanton and Highgate Springs, and illustrates his conclusions on a colored geological map, and also by means of a large plate of sections, which, together, will be of much service to future students of the region. The Taconic rocks are stated to be older than, and also unconformable to, the Potsdam sandstone. The apparent unconformabilities were explained by Logan on the ground of faults and displacements (Geol. of Canada, 1863, pp. 844–861), and this has since been the generally accepted view. But Mr. Marcou reaches different conclusions, and, by means of the idea of colonies, rids the subject, to his satisfaction, of adverse paleontological evidence. The Georgia slates contain the Primordial trilobites. He describes, as next above, the Phillipsburgh group, and this as passing above into the Swan-

ton group, and both series of slates as including lenticular masses or beds of limestone. These limestone masses contain the colonies, and from the fossils they afford he concludes—taking one of his lines of limestone beds as an example—that Billings's species *Lituites Farnsworthi*, *L. Imperator*, *Nautilus Pomponius*, *Murchisonia Vesta*, *Metoptoma Niobe*, *M. Orithya*, *Pleurotomaria postuma*, *Maclurea matutina*, *M. ponderosa*, *Ecculiomphalus Canadensis*, *E. intortus*. and *E. spiralis*, are part of the American *Primordial* fauna; and to the same category he refers also, for a like reason, species of *Asaphus*, *Cheirurus*, *Calymene*, *Illænus*, *Trinucleus*, *Rhynchonella*, *Murchisonia*, etc. Thus the precursor species are the *actual* species of the later Lower Silurian, colonized in the remote Cambrian before the era of the Potsdam sandstone. This application of the idea of colonies makes a jumble of the early Paleozoic of America, instead of indicating the way out from difficulties in certain regions of faulted and flexed metamorphic rocks. No such scheme can take from Calciferous, Chazy and Trenton fossils their value as tests of geological age, even if the question as to the Taconic slates is involved therewith, unless it first be substantiated by the study of the fossils in undisturbed strata.

3. *Volcanic Eruption on Hawaii.* A letter from the Rev. Titus Coan to one of the editors, dated Hilo, Aug. 24, 1881.—The stream of lava continued until it had reached within one mile of the sea, and three-fourths of a mile of a well-peopled part of Hilo. All at once the flow seemed to be checked, and, by the 10th of this month, little or no vapors were seen along its channel, or high up on the broader part of the stream, or about the summit of the burning mountain. The blackened lavas of the eruption cover about two square miles to an average depth of twenty-five feet; but this is only a rough estimate for no exact measurements have as yet been made. We judge the length of the third stream to be fifty miles, including all its deflections, and for the most of this distance it was, to all appearance, a surface stream.

4. *Glacier Scratches in Goshen in Northwestern Connecticut.*—Glacier scratches over the higher portions of New England are of special interest because they give the direction of movement in the ice free from the swervings due to the courses of valleys. The higher lands of Goshen are particularly favorable in this respect. Observations have been recently made by Mr. Henry Norton, of Winsted, which we here cite.—On the west side of the mountain (allowance having been made for magnetic variation), S. 41° E., but with one deep one, S. 77° E.; farther south, in Mr. McElhane's lot, several deep groovings S. 38° E.—and pointing, northwestward, directly toward Mt. Everett; south of the house, on the same lot, S. 22¾° E. and S. 58° E.

5. *On the Structure and Affinities of the Genus Monticulipora and its Subgenera, with critical descriptions and illustrative species;* by H. Alleyne Nicholson, Prof. Nat. Hist. Univ. St. Andrews. 240 pp., large 8vo, with six plates and many wood cuts. 1881. Edinburgh and London. (Wm. Blackwood & Sons.)—This vol-

ume, while not, as the author says, a monograph of this group of fossil corals, contains a historical and critical review of previous memoirs and conclusions on the subject, a discussion of the synonymy as to genera and species; explanations of microscopic structure; inferences as to the affinities and zoological position of the genus, and descriptions of several new species. The observations are based mainly on specimens collected in the United States and Great Britain. The volume bears evidence of much study and research in its preparation, and of liberal expenditure by the publishers in its manufacture, and will be welcomed especially by American paleontologists.

6. *Ulexite in California;* Note by W. P. Blake. (Communicated.)—Ulexite occurs in quantity in Kern County, California, in the bed of an extensive "salt marsh," a few miles north of Desert Wells, and twenty miles from Mojave Station on the railway.

7. *Worked Shells in New England Shell-Heaps;* by Edward S. Morse.—Mr. Morse called attention to the fact that heretofore no worked shells had been discovered in the New England shell-heaps. A similar absence of worked shells had been noticed in the Japanese shell-heaps. Worked shells were not uncommon in the shell-heaps of Florida and California. Mr. Morse then exhibited specimens of the large beach cockle (Lunatia) which showed unmistakable signs of having been worked. The work consisted in cutting out a portion of the outer whorl near the suture. To show that this portion could not be artificially broken he exhibited naturally broken shells of the same species, both recent and ancient, in which the fractures were entirely unlike the worked shells.—*Abstract of paper read before the Amer. Assoc. at Cincinnati.*

8. *Changes in Mya and Lunatia since the deposition of the New England Shell-Heaps;* by Edward S. Morse.—This communication embraced a comparison between the shells peculiar to the ancient deposits made by the Indians along the coast of New England, and similar species living on the coast at the present time. He referred to similar comparisons which he had made in Japan, wherein he had found marked changes to have taken place; changes which showed that the proportions of the shells had greatly altered.

He had made a large number of measurements of shells from a few shell-heaps of Maine and Massachusetts, and had obtained very interesting results. The common clam (Mya) from the shell-heaps of Goose Island, Maine, Ipswich, Mass., and Marblehead, Mass., in comparison with recent forms of the same species, collected in the immediate vicinity of these ancient deposits, showed that the ancient specimens were higher in comparison with their length than the recent specimens.

A comparison of the common beach cockle (Lunatia) from the shell-heaps of Marblehead, Mass., showed that the present form had a more depressed spire than the recent forms living on the shore to-day, and this variation was in accordance with observations he had made on a similar species in Japan.—*Ib.*

9. *Beiträge zur Morphologie und Physiologie der Pilze, Vierte Reihe;* by A. DEBARY and M. WORONIN.—After an interval of not far from ten years, the important series of papers by DeBary and Woronin, published under the above title in the Abhandl. Senckenb. Gesellsch., is continued in a fourth part which contains a paper by DeBary on *Investigations on the Peronosporeæ and Saprolegnieæ and the formation of a Natural System of Fungi.* The article covers 137 quarto pages, with six lithographic plates. The subject is treated under sixteen different heads, of which the first twelve are devoted to an account of different forms of *Pythium*, *Phytophthora*, *Peronospora*, *Saprolegnia*, *Achlya* and *Aphanomyces.* With the exception of some hitherto undescribed species, the writer has confined himself principally to the changes which occur in the formation of the oogonia and antheridia, giving with great minuteness the details of the process of fertilization. In the genera like *Pythium* and *Peronospora* where only one oospore is produced in an oogonium, the oospore is separated from the oogonium wall by a layer of protoplasm to which DeBary gives the name of periplasma, and he thinks that the markings formed on the outer coat of certain oospores is formed directly from the periplasma, and is not an exudation from the cellulose wall of the spore itself. In *Pythium* a small process, or befruchtungsslauch, penetrates the oogonium wall, and reaches the oospore. In this process DeBary distinguishes a thin homogeneous layer lining the wall, which he calls periplasma, while to the thicker axial portion he gives the name of gonoplasma. The act of fertilization consists, in *Pythium*, of the escape of the gonoplasma through the open end of the process and its union with the oospore. In *Phytophthora* and *Peronospora* during the act of fertilization some of the contents of the antheridium pass apparently into the oospore, but the transfer is by no means as marked as in *Pythium*, and the matter which escapes from the antheridial process consists of only a few granules and the whole axial portion does not escape as in *Pythium.*

In *Saprolegnia* and *Achlya* nothing could be seen to be discharged from the antheridial tubes and the fertilization consists merely in the contact of the male filaments with the surface of the oospores. Contrary to the view advanced by Pringsheim, DeBary finds that the thin spots in the oogonium walls of some species, and the papillæ found in others, have no direct connection with the antheridial tubes which may penetrate the oogonium walls in any place. It has long been known that in some of the *Saprolegneæ*, oogonia are found in which the oospores apparently ripen, although antheridia are wanting. It has been suggested that in such cases antheridia were actually present but had been overlooked. DeBary agrees with Pringsheim in affirming that in some cases oospores ripen without the presence of antheridia. He differs with Pringsheim, however, in considering such forms to be distinct species rather than accidental variations of species in which antheridia normally occur. He does not deny that forms

with antheridia and forms without them may have originally been derived from a single species, but cultures continued for two years showed that forms without antheridia constantly reproduced themselves, and they are, according to DeBary, instances of apogamous reproduction.

The fifteenth section treats of the systematic position of the *Peronosporeæ* and *Saprolegnieæ*. In the former order is included *Pythium*. The last section, to which, in a certain sense, all the others are merely introductory, is a valuable discussion of the relation of the different orders of fungi to one another, and to some extent of the algæ. Apparently, DeBary is not willing to go as far as Sachs in giving up the general distinction of algæ and fungi, although recognizing their close relationship. Starting with the *Peronosporeæ*, he considers that a series can be formed, on the one hand, by that order, the *Ascomycetes*, and the *Uredineæ*, the last named order being connected with the *Basidiomycetes* by the *Tremellini*. A second series is formed by the *Saprolegnieæ*, *Chytrideæ* and *Ustilagineæ*. With regard to the sexuality of Fungi, DeBary expresses himself in rather a conservative manner and considers that in some cases, as in certain *Ascomycetes*, sexual reproduction seems to be out of the question, and he is inclined to regard the spores in several groups to be of apogamous origin.

W. G. FARLOW.

10. *Fauna und Flora des Golfes von Neapel; IV Monographie: Corallina;* by Professor SOLMS-LAUBACH. Leipzig, 1881.—This small folio of 64 pages with three lithographic plates is the first botanical contribution which has been issued in the form of a separate memoir, although several botanical papers have appeared in the Mittheilungen of the Zoological Station at Naples, and Reinke has published two papers on the *Cutleriaceæ* and *Dictyotaceæ* of the Bay of Naples in the Nova Acta. Twenty pages are devoted to an enumeration of the Corallines in the region of Naples; including five genera, and twenty-five species. The specific account is followed by a chapter on the conformation of the organs of vegetation as a basis of generic distinctions. It is incidentally stated that the so-called heterocysts described by Rosanoff in *Melobesia farinosa* are really the spots from which hairs are given off, and according to Solms they are found also in *M. callithamnioides* and *Lithophyllum insidiosum*. The third chapter contains a minute account of the development of the fruit of *Corallina mediterranea*, with notes with regard to the fruit in some other species. The present writer does not accept the account given by Thuret of the difference in the cystocarps of *Corallina* and *Jania* but unites the two genera. In regard to the spermatozoids he maintains in opposition to Thuret that they are not naked but have a distinct wall comparable to that of the spermatia of fungi. The spores are borne on what Solms calls a fusion-cell, a structure found in all the order examined. The closing chapter has observations on the fructification of *Amphiroa*, *Melobesia*, *Lithophyllum* and *Lithothamnion*. An interesting account is given

of the thallus and fruit of *M. Thuretii*, the curious parasite on species of *Corallina*, and a similar parasite, *M. deformans*, is described by Solms from Australia. A formation of gemmæ, not elsewhere known in the order, is described and figured in *Mel. callithamnioides*. W. G. F.

11. *The Botanical Collector's Handbook*; by W. WHITMAN BAILEY. (G. A. Bates, Salem.)—This volume forms number three of the Naturalist's Handy Series, and contains full directions for the collection of all kinds of plants and their proper preparation for, and the arrangment in the herbarium. The writer has been aided in his account of the method of collecting cryptogams, by notes from experts in different departments, and there is a chapter by Mr. C. H. Peck on the preparation of fungi. At the end is a short account of the principal public herbaria in this country and a list of books relating to the floras of different countries. The book is illustrated by wood-cuts. W. G. F.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Ancient Japanese Bronze Bells*; by EDWARD S. MORSE.—Mr. Morse described the so-called Japanese bronze bells which are dug up in Japan. These bells had been described and figured by Professor Monroe in the Proceedings of the New York Academy of Sciences. Mr. Kanda, an eminent Japanese archæologist had questioned their being bells from their peculiar structure. Mr. Morse had seen a number of different kinds of bells, some of considerable antiquity, but none of them approached these so-called bronze bells. Mr. Kanda had suggested that they were the ornaments which were formerly hung from the corners of pagoda roofs. But the fact that none of them showed signs of wear at the point of support, rendered this suggestion untenable. Mr. John Robinson, of Salem, the author of a work on Ferns, had given the first suggestion as to the possible use of these objects. He had asked why they may not have been covers to incense burners.

Curiously enough old incense burners are dug up which have the same oval shape that a section of the bell shows. The bell has openings at the base and also at the sides and top, so that the smoke of burning incense might escape. It is quite evident that these objects are neither bells nor pagoda ornaments and this suggestion of Mr. Robinson's may possibly lead to some clue regarding their origin.—*Abstract of paper read before the Amer. Assoc. at Cincinnati.*

Primitive Industry, or Illustrations of the Handiwork in Stone, Bone and Clay of the Native Races of the Northern Atlantic Seabord of America, by Charles C. Abbott, M.D. 560 pp., with many illustrations. Salem, Mass., 1881. (George A. Bates).—A notice of this excellent work, and also of the following, is deferred to another number.

Report on the Geology and Resources of the Black Hills of Dakota, by H. Newton and W. P. Jenney: U. S. Geographical and Geological Survey of the Rocky Mountain Region, J. W. Powell in charge. 566 pp. 4to., with plates and a folio atlas. Washington, 1880.

APPENDIX.

ART. XLIV.—*Appendix to Paper on the Geological Relations of the Limestone belts of Westchester County, New York;* by JAMES D. DANA.*

3. *The rocks and their observed positions in Westchester County and New York Island.*

IN the following notes, the abbreviations used are: Av. Avenue; St., Street; calc., calcareous; *l.* limestone; *gn.*, gneiss (variety without excess of mica); *thin gn.*, thin schistose gneiss; *m.* micaceous, mica; *m. gn.*, micaceous gneiss; *m. sch.*, mica schist; *hard gn.*, hard or compact thick-bedded gneiss; *hbl.*, hornblende; *hblc.*, hornblendic; N., north; S., south; E., east; W., west; R., river; R.R., railroad; var., varying. In giving the strike and dip, the words strike, dip, are omitted; N. 20° E., 70° E. signifies strike N. 20° E., dip 70° to the eastward, and so throughout. As heretofore, the courses are corrected for magnetic variation. The courses and dip given are those corresponding to the T-symbols on the maps at the places mentioned; and where there is no T-symbol on the map, the course and dip is put in brackets. The maps referred to are that of Westchester County in volume xx, numbered Plate V, and that of Westchester County and northern New York Island, in volume xxi, numbered Plate xix.

1. ON NEW YORK ISLAND.

A. EAST OF 4TH AVENUE.—W. of 3d Av., 100 yds. from 4th, on 102d St., *gn.* and *m. gn.* N. 40° E., 90°, 80°–70° E., and again 70 yds. from 4th Av., on 102d St., N. 46° E. (varying), 80° E.; S.W. corner of 3d Av. and 103d St., *m. gn.* N. 22°–39° E., 70°–80° E., 90°, 80° W. to 60° W.; cor. Lexington Av. and 103d St., *m. gn.* N. 38° E., 65° E., N. 39° E., 85° E., with a twist to E. and W., and dip S. of 60°, much *hblc.* where beds most contorted.

Near 123d St. and Av. A, on East River, *m. gn.* N. 26° E., 60° W., the outcrop under water at high tide.—N.W. corner 120th St. and Lexington Av., *m. gn.* N. 26°–28° E., undulating; 122d St., E. of Lexington Av., N. side of St., *l.* N. 26°–28° E., 90°, 70°–45° E.; in E. part of open lot, bending to N. 55° E, (see above); [On 123d St., northeast part of same open lot *m. sch.* N. 31° E., 80° E.]; [W. side of Lexington Av., S. of 124th St., N. 26° E., 70°]. E. side of 4th Av., S. of 118th St., *l.* in *m. gn.* N. 27°–32° E., 90°, 80° W. S.E. corner of 130th St. and 4th Av., *m. sch.* and *gn.*, N. 26° E., undulating, dip 0°–90°, mostly 50°–70°.

B. BETWEEN 4TH AND 6TH AVENUES.—W. of 4th Av., on 102d St., S. side, *m. gn.* N. 21° E., contorted, 90°, 70° W., N. side 50 yds. from Madison Av., N. 29° E, 70°–80° W.; N. of 117th St., between Madison Av. and 4th Av., *gneissic l.* N. 28° E., 70°–50° E.; W. of Madison Av., *m. gn.* horizontal and undulating; W. of W. corner of 5th Av. and 120th St., S. of Mt. Morris Park, garnetif. *m. gn.*

* This paper is contained in volumes xx to xxii of this Journal.

N. 28° E., 50°-65° W.; 120th St., S. of S.W. angle of same Park, 400 feet E. of corner, *m. gn.* N. 17°-22° E., 45° E., and 250 feet E. of corner, 50°-70° W.; W. of 4th Av., either side of 120th St., *m. gn.* contorted, N. 27° E., undulating, and S. of 125th St., *m. gn.* N. 26° E., 65°-70° E.

In Mt. Morris Park, *m. gn.* N. 30°-32° E.; N. 30°-34° E.; 60°-70° E.; N. 45° E., 60°-70° E.; [also in S. part, fronting 5th Av., *m. gn.* N. 30° E., 75°-80° E., also in zigzags].

Between 131st St. and 133d St., N. side of 132d St., *l.* N. 20°-28° E., dip undulating, E. and W. but mostly E., and S. side of 132d St., *l.* over open lot and half way to 131st St., N. 24°-28° E., 90° or nearly; and to E. *m. sch.* N. 23°-32° E. (28° average), undulating, large contortions.

C. Between 6th and 8th Avenues.—In Central Park N. 32°-27° E., 70°-75° E., but much contorted. Along 7th Av., at S.E. corner of 138th St., *m. gn.* N. 34° E., 70° E.; near 139th St., N. 37° E., 65°-70° E.; 140th St., N. 30° E., 70° E.; near 145th St., N. 26° E., 80° E.; N. of 149th St., N. 26°-28° E., 80°-85° E.; [also, near 154th St., *gn.* and *m. gn.* N. 31°-33° E., 70°-80° E., 90°].

D. Between 8th and 10th Avenues, South of 155th Street.—On 9th Av., near 104th St., *m. gn.* N. 29° E., 10°-60° E.; E. of 9th Av. on 110th St., N. 27°-32° E., 80° E.; W. of 9th Ave. on 110th St.. *m. gn.* N. 34° E., 80°-90°, contorted and much *hblc.*

East side of rocky part of Morning Side Park in line of 115th St., *m. gn.* N. 22°-29° E., 65° W.-90°, *hblc.* layers; same, farther N., nearly to line of 117th St., N. 37°-40° E., 60°-75° E., large slabs cleaved off and slid down the bluff; [same, in line of 117th St., N. 34°-37° E., 70°-80° W. to 90° and 85° E.;] same, in line of 118th St., 70 yds. W. of 9th Av., N. 42° E., 90°, 80° E. to 80° W. Just W. of Morning Side Park, five observations commencing at the most southern, *m. gn.* N. 24° E., 60° W.; N. 29° E., 80° W. to 90°; N. 34° E. 90° ±; N. 33° E., 90° ±; N. 33° E., 90° ±.

On St. Nicholas Av. and 125th St., N. 28° E, 90° ±; same Av., along Convent Grounds, between 126th and 129th Sts., N. 27° E., 90°, some *hblc.* and near 126th St. granite veins, and above 129th St. N. 30°-31° E., 70° W., var. to 60° W. On S. part of Convent Grounds N. 22° E., 50° E. On S.W. part of Convent Grounds, three observations, N. 22° E. average, 65° W., var. to 50° and 70° W.; W. part of same grounds, near the fence, *m. sch.* and *m. gn.* N. 30° E., 70° W. Near N. end of same grounds N. 32° E., 70°-60° E., and near its middle, *m. gn.* N. 27° E., 70°-75° E. On St. Nicholas Av., near 138th St., *m. gn.* and *m. sch.* N. 33° E., 80° W. to 80° E., mostly E.; near 144th St. N. 32°-37°-27° E., 70° E., much contorted; near 145th St. N. 26° E., 90° and to E. of last on 145th St., N. 30° E., 90°, 80° E.

E. Between 8th and 10th Avenues, North of 155th Street.—At 156th St. N. 14° E., 70°-80° W., N. 28° E., 75° W.—161st St., N. 19° E., 80°-65° W.—Between 161st St. and reservoir, N. 24° E., 80°-65° W., N. 38° E., 80° E. to 90°, N. 19° E., 80° W.—Near river, below reservoir, N. 20° E., 70° W.—Within 120 rods N. of reservoir, along 10th Av., N. 28° E.. 70°-80° E.; N. 29° E., 65°-70° (varying to 50°) E.; On slope toward river, N. 30° E., 80° W. to 90°; N. 30° E., 90°; N. 37° E., 80° E. to 90°.—Between 120 and 180 rods N. of reservoir, along 10th Av., N. 19° E., 80°-85° E. (var.), N. 23°-24° E., 80° E. to 90°; On slope toward river, N. 27° E., 90° E.; N. 38° E., 80°-70° W.; N. 30° E., 90°; N. 31° E., 80° W.—Farther N. on 10th Av., S. of Sherman's Creek, N. 21°-32° E., 80° E. to 80° W. (west side of road); large granite veins; N. 7°-22° E., 60° E. to 70° E. (east side of road); [west side of road nearly opposite, N. 22°-7° E., 50° E. to 65° W., and varying just south to 70° W. and 20° W.]; N. 55° E., 35°-30° E. (E. side of road); N. 83° W. to 72° E., 30°-40° E. (E. of road); N. 27°-32° E., 65°-70° W. (on top of ridge).—Farther N.E. toward Sherman's Creek, N. 8° W., 50° W. (40°-60°); N. 12° E., 65°-70° W.

F. On or West of 10th Avenue, South of 155th Street.—Near 10th Av. S. of 125th St., *m. gn.* N. 30° E., 80°-75° E.; N.E. corner of 130th St. and Broadway, N. 29°-30° E., 80° E. to 90°; On 10th Av., N. of 133d St., N. 26° E., 75°-65° E., and near 136th St., N. 32° E., 75° E. to 90°.

On 11th Av.. N.W. corner with 131st St.. under house, *m. gn.* N. 28° E., 80° E. to 90°; on 132d St., *m. gn.* N. 23°-25° E., 90°; W. of 11th Av., on N. side of 133d St., N. 23°-30° E., 90° to 8° W.—In "Park," near corner 10th Av. and

133d St., *m. gn.* N. 20° E., 90°, 80° E., and near 11th Av., N. 28° E., 70° E., and in line of 136th St., N. 39° E., 50° E. (varying to N. 55° W., 35° W.—On 11th Av., N. of 157th St., N. 30° E., 80° E. (average); in line of 137th St., N. 19° E., 70° W. to 80° E.; same Av. and 148th St., *m. sch.* to thin *m. gn.*, N. 25° E., 90°, 80° E.; same Av., but nearly half way to 10th, above 145th St., N. 28° E., 80°–75° E.; [same Av. and 147th St., E. side, N. 28° E., 85° W. to 85° E.].

G. ON OR WEST OF 10TH AVENUE, AND ON OR NORTH OF 155TH STREET.—On 11th Av. near 160th St., N. 22°–30° E., 70°–65° E.; [same Av. and 161st St., S.W. corner, N. 26° E., 90°–70° E.]; same Av., 162d St., N. 28° E., 90°–80° E.; [same Av., 163d St., N.E. corner, N. 29° E., 90°–80° E.]; same Av. and 164th St., N.E. corner, N. 29° E., 90°–80° E. Near junction of 11th Av. and Kingsbridge Road, N. 17° E., 90°. On Kingsbridge road and 165th St., *m. gn.* N. 29° E., 90°; same and 187th St., N. 29° E., 65° E. Near Hudson River, 155th St., 100 yds. off, *m. gn.*, N. 14°–22° E., 90°, 80° E. On Hudson R. railroad, nearly in line of 163d St., N. 22°–24° E., 70°–80° E., var. beyond to 55° E.; on R.R., W. of Deaf and Dumb Inst., dip 65° W.; on R. R., W. of Inst. for Blind, N. 24° E., 70° E.; on R.R., 100 yds. S. of Ft. Washington Station, *m. gn.* N. 22° E., 60°–70° E.; on R.R., just N. of Ft. Washington Station, N. 31° E. to N. 37°, 60°, 70°, 55° E.; on R. R., 150 yds. farther, *m. gn.* N., N. 20° E., 70° E. varying to 60° E.; on R. R., 500 yds. S. of Inwood Station, *m. sch.* N. 23° E., 70° E.; on R.R., 200 yds. S. of Inwood Station, *m. gn.* N. 23° E., N. 20°–30° E., 70°–65° E.; same, adjoining station, N. 34° E. Just E. of Inwood Station, *m. gn.* N. 49° E., 55°–65° E., but on N. side of road, N. 33° E., 60°–70° E.

H. NORTH OF SHERMAN'S CREEK AND INWOOD STREET.—W. of Farmer's Bridge over Harlem, S. of Kingsbridge, *l.* N. 12° E., N. 32° E., N. 37° W., best N. 32° E., dip 90° to 80° W.; going S., just S. of first brook-crossing, *l.* N. 24° E., 75° E. to 90°; 180 yds. farther S., *l.* N. 38° E., 75° E. to 90°; farther S., positions of next 4 T-symbols, on W. side Kingsbridge Road, *l.* N. 47° E., 50° to 60° E.; N. 60° E., 60°–65° E.; N. 75° E., 45° E.; N. 55° E., 75° E. Over 100 yds. W. of Kingsbridge Road, at loc. of the northern of 3 T-symbols, N. 50° E., 60° E.; at loc. of other 2, N. 67° E., 55°–65° E. (varying to N. 75° E.).

At Inwood St., a bed of *m. sch.* in *l.*, and E. side of Kingsbridge road, *m. sch.* N. 60 E., 60° E.; here 150 yds. E. of Kingsbridge road *l.* N. 58° E., 60° E.; farther S., W. of head of Sherman's Creek, *l.* N. 40°–43° E., 60°–70° E.; 350 yds. S. of head of Sherman's Creek *l.* N. 38° E., 65° E. (most southern outcrop observed).—On Inwood Parade Grounds N. of Sherman's Creek, 4 T-symbols, commencing with the easternmost, *l.* N. 7°–16° E., 70° E.; N. 22°–27° E., 70° E.; N. 17° E., 60°–65° E.; N. 48° E., 70° E. (At the more eastern outcrops a large granite vein in the limestone, having the course N. 15° E., corresponding with the strike of the enclosing *l.* and also an intercalated bed of *m. sch.*, having the strike N. 12°–32° E., dip 70°–80° E.

2. OUTSIDE OF NEW YORK ISLAND, IN WESTCHESTER COUNTY.

ON OR NEAR LIMESTONE AREA NO. 1.

a. South of 148*th St. and E. of* 3*d Avenue.*—E. of Brook Av., in 133d St., *m. gn.* N. 24°–32° E., 80° E. to 90°; on St. Ann's Av. in 136th St., *l.* N. 37° E., N. 32°–35° E., 80° W. to 80° E., near 138th St., *l.* N. 32° E., 80° E.; [in 140th St., E. of St. Ann's Av., *m. gn.* N. 26°–28° E., 90°]: between 142d and 143d St., *l.* N. 22° E., 75° E.; in 143d St., *l.* N. 17° E., 75° E.; in 146th St., N. 22° E., 90°.—Between Brook Av. and Willis Av., N. of 134th St., *m. gn.* about N. 22° E., undulating, 15°–20° E., and to eastward N. 37° E., 65° E., and N. of 135th St., *m. gn.* N. 37° E., 40° W., varying much; N. of 136th St., near the *l.* belt, *gn.* N. 31°–37° E., 80° W. to 80° E.; between 139th and 140th St., N. 24° E., 75° E.

Port Morris, E. of R.R., *m. gn.* or *sch.* with many granite veins and much contortion. best N. 29° E., 70°–75° E.; W. of R.R., same, N. 27° E., 75° W.—N. of Port Morris, E. of Boulevard on branch R.R., *m. sch.* or *gn.*, N. 28°–47° E., 60°–70° W, but varying much; still many granite veins; W. of Boulevard, W. of last, S. of 144th St. on R.R., N. 28°–24° E., 60°–70° W.

b. South of 148*th St., and West of* 3*d Avenue, about Mott Haven.*—E. of R.R., *l.* N. 26°-34°—15° E., 90°, 70° W., much contorted; [on S. side of N. ledge of *l.*, *m. sch.* N. 25° E., 75° W.]—W. of R.R., S. of station, ledge of *m. gn.* and *m. sch.*, N. 26° E., 60°-80° W.; on 138th St., W. of *l.*, *m. gn.* N. 24°-32° E., 60° W., [and 50 yds. farther N., N. 17° E., 80°-60° W.]—N. of Mott Haven Station, N. of 138th St., along R.R., *l.* N. 18°-20° E., 90° to 70° W., and 100 yds. N. of station, *l.* N. 20° E., 50°-70° W., *m. sch.* in *l.* rusting from pyrite present; on 144th St., W. of Mott Av., *m. gn.* N. 14° E., 60° W., and N. 22° E., 70° W.

c. Between 148*th Street and* 163*d Street.*—W. of Harlem R.R., between Mott Av. bridge over Hudson River R.R. and Harlem R.R., *l.* N. 15° E., 60° to 80° W. and E., *l.* extending 520 feet W. of R.R.; at the bridge and W. of it *m. sch.* and *m. gn.* much contorted N. 22°-32° E., 26°-80° W., local flexures; N. of bridge, *gn.* observed to be fibrolitic; 250 yds. N. of bridge on Mott Av., *m. gn.* N. 24° E., 70° W. [E. of Mott Av., 100 yds. N. of junction of the two R.R., in a small hill, *l.* N. 13°-18° E.]; E. of St. Ann's Av., near 149th St., N. 23° E., 90° to 60° W.

On Harlem R.R., below 158th St., *l.*, and near 160th St., N. 22°-27° E., 90°, to 30° W., E. of Harlem R.R., and W. of 3d Av., on 149th St., *l.* N. 22° E., 35°-45° W.; near 150th St., large undulations, N. 22° E., 0°-45° W. and E.; just below 155th St., W. side of Elton Av., *l.* N. 22° E., 0°, 20° to 30° W., and E. side of same Av., *l.* N. 22° E., 35° W. to nearly horizontal and 50° E.; on 156th St., E. side Elton Av., *l.* N. 19° E., 45°-25° E. and horizontal; above 159th St. on Elton Av., *l.* N. 18° E., 35°-60° W., and W. of Av., N. 22° E.—150 yds. E. of St. Ann's Av. near 149th St., *m. gn.* N. 23° E., 60° W. to 90° [161st St., *l.* N. 22°-28° E., 90°-30° W.]; [162d St., 100 yds. W. of Harlem R.R., *l.* N. 24°-26° E., 70° W.

d. North of 163*d Street, East of Harlem R.R.*—South of Tremont on 167th St., near Washington Av., *l.* N. 19°-15° E., 80° W. to 80° E.); on 166th St., 100 feet E. of Washington Av., *l.* N. 19° E., 70° E.; between 167th and 168th Sts., W. of same Av., *l.* N. 20° E, 70° E.; 40 feet E. of the Av., *l.* N. 26° E., 90° to 80° E.—Just E. of 3d Av. on Boston Av., *m. gn.* N. 21° E., varying to N. 27° E., 60°-70° E.; on 167th St., thin *m. gn.* N. 15° E., 70°-80° E.; on 169th St., N. side, just W. of Fulton Av., *m. sch.* or *gn.* N. 14° E., 70°-60° E., varying much.—In Tremont, W. of 3d Av., near 170th St., *l.* N. 27° E., 90°; on 172d St., N. 28° E., 90°; farther N., *l.* N. 21° E., 90° to 70° E.; To eastward on Locust Av., E. of 3d Av., *m. gn.* and *sch.* N. 18°-29° E., 90°-70° E. [E. of Bronx R. in West Farms, *m. gn.* N. 18° E., 70° E.].—Half way from Tremont to Fordham, E. of Fordham or 3d Av., *m. sch.* N. 21° E., 90°-75° E.

In Fordham, *l.* near R.R. station, E. side of Kingsbridge road, strike not distinct in the small outcrop.

N. of 163*d St. and West of Harlem R.R.*—In Fordham, on road going W., *gn.* N. 28° E., 90° to 80° E., and 200 yds. farther W., N. 28° E., 90°; S. of last on 1st road W. of R.R., gray *gn.*, with some *m. gn.*, N. 26° E., 70° E. and N. 29° E., 70° W.; ½ mile and farther S.W. just W. of same road, observations corresponding to 8 T-symbols in succession, N. 30° E., 70° E. and N. 30° E., 75°-80° W., the easterly predominating; N. 30° E., 70° E.; N. 30° E., 90° to 80° E. and N. 30° E., 90° to 80° W.; N. 26° E., 60°-75° W.; N. 29° E., 80° W.; W. of Tremont Station *gn.* partly *hblc.*, N. 22° E., 40°-60° W., varying to 30°.—S.W. of Tremont, just W. of limestone area, observations corresponding to 5 T-symbols, *quartzytic gn.* mostly, varying to *m. gn.* N. 24° E., 90°-70° E.; N. 27° E., 60°-40° W.; N. 21° E., 60°-30° W.; N. 31° E., varying W. dip.; N. 18° E., 70°-60° W., but varying much.

In Fleetwood Park, N.E. side, *m. gn.* undulating; in E. part of ledge, N. 28° E., dip 60° E., but just W. bends over and dip 45° W., then 50°-80° E. again; W. part of same ledge, N. 16°-22°-24° E., 75°-55° W.—In center of the Park, *l.* N. 24° E., 75° W.; 75 yds. more to W., *l.* N. 28°-30° W., 60°-65° W.; to N.W. of last, near schist of W. side of Park, *l.* N. 28°-30° E., 60°-70° W.; W. part of Park, *m. gn.*, near *l.*, N. 28° E., 65° W.; farther S.W., *m. gn.* N. 32° E., 55°-65° W.

W. of Fleetwood Park, near W. entrance, *m. gn.* N. 24°-26° E., 70°-80° W.; 200 yds. N.E. of last, on Av. adjoining Park, *m. gn.* N. 26° E., 70°-80° W.; same road farther N. in line with N. side of Park, *m. gn.*, N. 8° E., 80° E. to 80° W.—And

N. 13° E., 90° about; 250 yds. N. of Park, *m. gn.* N. 26° E., 85° W.; near N. end of this schist on Arcularius St. and same Av., *m. gn.* N. 20° E., 90° ±, and 50 yds. more to W., N. 19° E., 70° W. (T-symbol wrongly makes it E.).

Just N. of Fleetwood Park, *l.* (continuation of that of the Park), observations corresponding to 5 symbols, N. 22° E., 90°; N. 18° E., 90° ±; N. 18° E., 90°: N. 19° E., 70°-80° W., N. 19° E., 80° W.; the last near end of the gneiss a little beyond line of Arcularius line: here the limestone becomes that of Area No. 2.

AREA NO. 2 AND ITS VICINITY.—Commencing at the north ¼ mile N. of Manhattan House (M on map), on Central Av., W. side, *m. gn.* N. 32° E., 50°-65° E.; 300 yds. N.W. of same house on same Av., *m. gn.* N. 27° E., 65° E.; 100 yds. W. of same house on road going W., *m. gn.* N. 30° E., varying, 55°-60° E., and 400 yds. farther W., N. 30° E., 65°-70° E., varying; 400 yds. N. W. of Club House (C on map), *m. sch.* N. 28° E., 50°-60° E.; [800 yds. S.W. of same, N. 23° E., 50°-60° E.].—In Mt. Eden limestone region, 3 T-symbols, N. 12° E., 70°-80° W., (near a barn), N. 2° E., 60° W., N. 2°-12° E.; 45°-20° E., undulating: and to S.E., 4 T-symbols, N. 17° E, 60° W.; N. 12° E., 60° W., N. 27° E., 60°-65° W., N. 31° E., 70°-65° (the last near bottom of valley west of Mt. Eden region); [again, near last, N. 21° E.]

Near head of Cromwell's Creek (the bay E. of lower part of Central Av.). and where Central Av. crosses the brook, W. of brook, on road going N.W., *m. gn.* N. 12°-19° E, 60°-65° E.; N. 14° E., 55° E., some *hblc.* layers; [near the last, W. of road, near house, N. 21°-23° E., 65°-70° W.]; little to S. on Central Av., W. side, *m. gn.* N. 31° E., 68° E.—East of brook (and "Judge Smith's House"), *l.* N. 24° E., 40° E.; N. 27° E., 36°-40° E.; more to E. near limit of *l.*, *l.* N. 21° E., 60°-65° E.; and E. of last, *m. gn.*, N. 21° E., 65°-70° E. (the T-symbols of last two observations wrong in the stem pointing W.).—W. of middle of Cromwell's Creek, on Central Av., W. side, *m. gn.*, N. 23° E., 65°-75° E.: to S. of last, N. 32° E., 80° W. to 90°.

E. of S. part of Cromwell's Creek, on 1st St., *l.* N. 16° E., 90° ±; 225 yds. to N.E. of bridge, thick bedded *l.*, N. 16° E.; just E. of last, *gn.* N. 15° E., 80° W. to 90°.—W. of S. part of same Creek: at W. end of bridge, *m. gn.* N. 20° E., 45°-60° W.; on Central Av. below High St. near Case's Hotel, *m. gn.* much contorted N. 27° W. to N. 27° E., 30° W. to 65° W.; directly W., 150 yds. N. of McComb's bridge, *m. gn.* in zigzags N. 28° E., 80° W. to 80° E.; on Central Av. 150 yds. N.E. of same bridge, dip 60° to 80° W. On Hudson River, W. of N. end of same bridge, 75 yds. W. of the Av., *gn.* N. 27° E., 80° E. to 65° E., and 120 yds. to N., near same river N. 27° E., 75° E. to 90°.

AREA NO. 3 AND ITS VICINITY.—At Kingsbridge, on R.R., *l.* N. 37°-40°E., 60° E.; just W. of the limestone area, on R.R., *m. sch.* or *m. gn.*, partly *hblc.* N. 42° E., 65°-70° E.; 250 yds. W. near bridge at W. side of the bend in Sp. Duyvil Creek, *m. gn.*, N. 56° E., 70°-60° E. (here layer of actinolite, etc.). W. side of Tibbit's Brook, 4 to 5 miles N. of Kingsbridge, *m. gn.* N. 18°-24° E., 60° E.

AREA (NO. 3A) NORTH OF SPUYTEN DUYVIL TO RIVERDALE, ON THE HUDSON.—At Spuyten Duyvil, near old R.R. station, *m. gn.* N. 40°-75° E., 35°-60° E., contorted, varying widely; 60 yds. S., N. 50°-37° E., 45°-50° E.; 50 feet N. of forking of R.R. above same R.R. station, *m. gn.* and *gn.* N. 47° E., 70° E.; about 200 yds. N.N.E. of last, and as far from river, *m. gn.* with some *l.*, N. 47° E., 60° E.; on Whiting estate, S. of E. of an old limekiln, *l.* quarried, N. 17° E., 70° E., but varying in strike and dip; 250 yds. N.E., *gn.* of E. side of *l.* area, part *hblc.*, N. 4°-8° E., 55°-60° E.; [S. of Delafield's, *gn.*, part *hblc.*, N. 10° E., 60° E., 55°-65° E.]; W. of same, large quarry of *l.*, part tremolitic, N. 10°-18° E., 60°-70° E.; E. of Riverdale station, N. of *l.* area, *m. gn.* N. 30°-34° E., 65° E.; [S.E. of Mt. S. Vincent grounds, quarries of *m. gn.*, N. 34° E., 60°-55° E.; on R.R. near Mt. St. Vincent depot, N. 17°-22° E., 55°-65° E.].

AREA 4 —S. of W. Mt. Vernon ⅜ mile, W. side of R.R., *l.* N. 29° E., 70° E.; E. of R.R., *l.* same; E. of *l.*, *thin gn.*, same; on ridge between W. Mt. Vernon and Mt. Vernon, *thin gn.* N. 21°-25° E., 70°-75° E.; on N. Y. R.R., below Mt. Vernon, *thin* or *m. gn.* N. 27° E., 65°-70° W.; at W. Mt. Vermon, E. of R.R., *m. gn.*, N.

20° E., 65°–75° W., and W. of Bronx River near Williams Bridge, thick *gn.* N. 20° E., 90° to 80° W.; *m. sch.* and *m. gn.* on W. side of R.R., S. of Woodlawn, N. 18° E., 82° E.; at Williams Bridge, W. of R.R. track, *m. sch.* N. 20° E., 60° E., and E. of river, same.

Area 5.—In New Rochelle, N. of Davenport's Neck and of the Serpentine loc., thin *m. gn.* N. 19° E. and N. 27°–38° E., 85° E. to 85° W.; on S.E. and S. shore of Davenport's Neck, S. of Serpentine loc., *m. gn.* N. 27°–43° E., 70°–85° E.; between N. Rochelle and Mt. Vernon, to the E., *m. gn.* (but partly whitish gneiss), to the W., *m. sch.* and *m. gn.*, N. 20° E. (average) 90°, the 8 T-symbols on map correspond, commencing on the east, to N. 25° E., 90°; N. 24° E., 90°; N. 21° E., 85° W., N. 20° E., 85° W.; N. 27° E., 85° W.; N. 15°–25° E., 90°–80° W.; N. 20° E., 78° W.; N. 21°–25° E., 70° W.

In Marmaroneck, *m. gn.*, near R.R. depot, N. 37° E., 70°–85° W.; the 4 T-symbols to N.W. correspond to N. 39° E., 80°–85° W., rock same; N. 37° E., 90°. 80°–85° E., *hard gn.*; N. 29° E., 80°–85° E., *hard gn.*; N. 29° E., 80°–85° E, same *hard gn.*; N.W. of last, in Scarsdale, *m. gn.*, N. 17° E., 90°–70° W.—South of New Rochelle the 7 T-symbols correspond to: *m. sch.* or *m. gn.* N. 24° E., 90° and 85° E. to 85° W.; same, dip 80° E.; N. 22°–32° E., 70°–80° E., much contorted; N. 18° E. (average), 80° E.; N. 24°–30° E., 90°, 85° E.; N. 27°–30° E.; N. 37°–47° E., 90°±; N. 30° E., 90°; N. 24° E., 90° to 80° E.; N. 20°–23° E., 80° E. to 90°.

Area 6.—At Portchester, thin *m. gn.* (brilliant mica scales white and black, white predominating), N. 10°–12° E., 65°–70° E.; N. of Portchester, *m. gn.* N. 10°–18° E., 70°–75° E.; N. 7°–12° E., 60° E.; W. of Serpentine, N. 11° E., 90°, N. 21°–27° E., 75°–80° W., N. 14° E.—3 miles N. of Portchester, same *m. gn.* N. 37°–67° E., 22°–42° W.—W. of Glenville. same brilliant *m. gn.*, N. 32° E., 30° W.; N. 37°–76° E.; 30°–50° W.; N. 37° E., 30° W.—N. of Serpentine area *m. gn.* N. 37° E., 30°–35° W.; S. of Serpentine area, N. 17°–58° W., 30°–50° E., but with great contortion var. to N. 24° W., dip 30°–45°–50° E.; farther S., just N. of R. R., N. 20° E. (but var. much), 65°–70° E.

Areas 7, 8.—*m. sch.* or *m. gn.*, E. of Yonkers, N. 16° E., 70° E.; N. of last, W. of river, and of N. end of cemetery, N. 12°–37° E., 55°–60° E.; ¼ mile more to N., N. 32° E., 60°–70° E.; Yonkers to R.R. south of Bronxville, the 5 T-symbols, thin *m. sch.* N. 15° E., 65° E.; *gn.* N. 27° E., 85° W.; same, N. 27°–30° E., 65°–70° W.; thin *m. gn.* N. 24°–27° E., var. to N. 14° E. and N. 38°E., 90° to 85° W.

Area 9.—*l.* N. 10° E., 86° W.; W. of *l.*, thin *m. gn.* N. 18°–19° E., 85° E.–85° W.

Area 10.—The Tuckahoe belt extends little S. of bridge at Bronxville; E. of river, S. and N. of R.R. depot, *hard gn.*, contorted, N. 23°–26° E., 90°–70° E., 85° W.; W. side of river, near bridge, *gn.* N. 20° E., 60° W.; ½ mile N. of depot, on R.R., *gn.* N. 23° E.; W. of this point in the marsh of the valley, *l.* N. 26° E., 90°; 50 yds. N., *l.* outcrops E. of R.R.—West of Bronx R., between Bronxville and Tuckahoe depot, hard *gn.*, N. 20°–24° E., dip W.; 1st, *l.* quarry N. of Tuckahoe depot, *l.* N. 35° E., 65° W.; E. of *l.* is *m. sch.*, N. 35° E., 60°–70° W.

Area 11.—In Scarsdale, along R.R., N. and S. of depot, *l.* N. 26°–28° E. [also N. 10°–35° E.]. 55°–60° W.; ½ mile E. of depot., *hard gn.*, and some *hbl.* schist, N. 26° E. (var. to 40°), 60°–80° W.; again, ¾ mile S.E. from depot *m. gn.* N. 28° E., dip 65° W. The *l.* N. of depot a very narrow strip; narrow valley continues toward Hartsdale. ½ mile N. of Scarsdale depot, W. of river, *l.* near river, which continues N. through Fox Chapel Garden grounds, 75–100 yds. wide; *granitoid gn.* E. of *l.*, N. 18°–28° E., dip 60°–65° W.—In Hartsdale, in road going E. from depot, *l.* N. 18° E., 45° W., but ½ mile E. of depot, *m. sch.*, N. 18°–24° E., dip 60°–65° W. and ¾ mile W., whitish *gn.*, N. 17°–19° E., 60° W.—From Hartsdale nearly to White Plains a narrow marsh along east side of river.

Area 12.—1½ mile N. of White Plains. outcrop of *l.* from beneath stratified drift, N. 13° E., 46° W.; 400 yds. N. of railroad station at White Plains, banded *gn.*, partly *hblc.*, N. 30°–37° E., but var. to N. 42°, 56°, 72° E.; 2 miles E., on road to Rye Pond, firm *gn.*, N. 22° E. to N. 50°–60° W.; ⅛ mile farther N.E., reddish granitoid *gn.*, N. 27°–42° E., 50° W.; nearer Rye Pond, gray *gn.* much contorted.

AREA 13.—East of Dobbs Ferry, hard *gn.* N. 3°–22° E. and N. 11° E., 90°; at Ashford, *hblc. gn.* N. 25° E., 60°–70° E.; ⅛ mile E. of Hastings, hard *gn.* N. 17° E., 90°–80° E.; 1 mile E. of Hastings, near Sawmill R., *gn.* N. 24° E., 65° E.

AREA 14.—About 3 miles N. of Ashford *l.* commences, the rock in the valley there dark gray *gn.*; ½ mile N. of this, *l.* N. 22° E., 80° E.; at E. Tarrytown, ½ mile E. of river, on E. border of the broad valley, *l.* N. 32° W., 45° E.; again N. 28°–33° E., 90° to 80° E.; W. of river, is compact *gn.*, contorted, N. 27°–32° E., dip 60° E.; E. of river, 1 mile N. of E. Tarrytown, a bluff of *l.* to east of road, N. 47° E., dip 70° W.; ¼ mile S.W. of Unionville *l.*, N. 24° E., 70° W. to 70° E., the latter prevailing.

AREA 15.—275 yds. S. of Unionville depot, E. of R.R., *l.* N. 24° E., dip 75° E. In Pleasantville, S. of R.R. depot, *l.* N. 37° E., 60° to 80° E., here an intercalated bed of *m. sch.*; 50 yds. N. of depot, *l.* N. 30°–34° E., 90° to 70° W.; ¾ mile E. of depot, on "Broadway," *l.* N. 18° E., 80°, and more to N. dip 40° E.; E. of *l.* ¼ mile, thin *gn.*, N. 40°–43° E., 90° to 80° E. At Chappaqua, *l.* about 250 yds. in width. 1½ miles E. of Pleasantville depot, an outcrop of *l*, N. 3° E., 50° W., area small; W. of *l.*, *m. gn.* N. 12° E., 70° W.; 1¼ mile S.E. of *l.* toward Armonk, grayish *gn.* N. 9° E., 55° W.

AREA 16.—S. of Sing Sing R.R. station, *l.* N. 37° E., dip 55° E., again N. 30°–42° E., 70°–80° E.; near N. end of prison, *l.* N. 32° E., 40°–50°–60° E.; E. of prison, on Spring St., *l.* N. 24° W., 20°–30° E. Abreast of station, *m. sch.* N. 25°–40° E., 70°–80° E.; S. of *l.* area. in Scarborough, *m. gn.* N. 23°–40° E., 50°–60° E.—Along brook near entrance to Dale Cemetery, *l.* N. 20° E., 40°; ½–¾ mile E. of last, near road to Camp Woods, *l.* N. 64° E., 40° E.; ¼ mile N. of entrance to Cemetery, on road, *m. sch.* N. 54°–74° E., 90° to 80° E., contorted; in field 60 yds. W. of last, same *m. sch.* N. 30°–38° E., with *l.* either side N. 37° E. —W. of Dale Cemetery, above junction of Post road with road next E., *l.* N. 32° E., 50° E., and W. on aqueduct, *l.* N. 58° E., with bed of granitoid *gn.* N. 58° E., 65° E.

AREA 18.—At Croton, 1 mile E. of R.R. station, 250 yds. S. of Barlow's, *l.* N. 24° W., 60° E.; near last, ¼ mile N.E. of Episcopal Church, fine-grained *l.* quarried, but bedding indistinct; ¼ mile farther N.E., *m. sch.* N. 2° E., 60° E.

AREA 19.—¼ mile S. of Croton R. at Huntersville, *l.* N. 52° E.; W. of *l.*, rusting *m. sch.* N. 52° E., 90°–80° E.; again S.W. for ½ mile, *l.* N. 52°–60° E.; toward Quaker Bridge, *l.* N. 77° E., 85° E. to 90° and 85° W.

AREA 20.—At Merritt's Corners, *l.* N. 36°–44° E., 60° E.; 1 mile N., *gn.* N. 42° E., 70° E.

AREA 21.—On east side of Croton Lake, coarse cryst. *l.* mostly N. 7°–22° E., contorted, average N. 14° E., 60° W.–90, *l.* contains graphite; *gn.* on east side of *l.*, contorted (mica black), N. to N. 30° E. average N. 14° E., 55°–60° W.; *l.* extends into lake.

AREA 22.—E. and S. of depot, *l.* much contorted, N. 12°–40° E., dip W.; 200 yds. S., N. 62°–67° W.; dip E. farther south, N. 40°–42° E., dip W.; on hill to E., *m. sch.*, with granite, N. 40° E., 50° W., again, *m. sch.* and *gn.* N. 37°–42° E., 45° W.

AREA 23.—1 mile N. of south extremity of area, *l.* N. 8° W. to N. 10° E., 60°–65° W., varying to 40° W.; *gn.* in contact with *l.* and conformable, the *gn.* partly hard feldspathic; W. border of river-valley, *gn.* N. 5°–11° E., 40°–60° W. Near S. end of area, and for 1 mile E, *m. sch.* and *thin gn.* N. 10°–11° E., 40°–60° W.—East of Armonk, E. of Byram R., *l.* N. to N. 5° E., W. 60°; ½ mile S.W. of Armonk schistose *gn.* N. 15° E., 55° W. [Between this point and Kensico depot, going S.W., N. of Kensico village, hard whitish to reddish gneiss, N. 12°–22° E., 90°±; N. 14°–40° E., 90°–80° W.; S. of Kensico village, hard gray *gn.* (black mica) N. 12° E., 60° W., same, N. 14°–27° E.].—Near N. extremity, just W. of Byram Lake, twisted *gn.*, N. to N. 60°–38° W., N. 7° E.; N.E. border of lake, thin whittish *gn.* N. 8° W., N. 22° E., 45° W., contorted, again thin *gn.* N. 2° E., again dipping under last thick bedded feldspathic partly banded

and gneissoid; again ½ mile E., thin *gn.* N. 8° W., 40°-45° W., some *hbl.* portions. No *l.* in sight, being two feet under water in lake.

AREA 24.—S.W. of area, *gn.* N. 24° E., 40° W.; S.E. of area, *gn.* N. 8° W., 50° W.; W. of valley, 1 mile N. of S. end of area, *m. gn.* (mica black), N. 8° W. to N. 22° E., varying to N. 67° E., 10°-35° W. In Bedford village, N. margin of area, *l.* N. 57° E., 40° W.; near head of Mianus R., *l.* N. 84° E., 60° N., varying to N. 48° W., 40° W.; just to E., 300 yds. S. of road, *l.* N. 67° E., 55° W., and E. side of same low hill, consists of *granulyte* (cream-colored orthoclase and a few garnets), N. 57° E., 65°-70° W.; 300 yds. E. of last, after passing the granulyte, more *l.*; limestone valley here fronted to N. by a high, nearly E. and W., precipice of bedded *gn.*, strike of *gn.* N. 62° E., 25° W. 1¾ mile E. of Bedford village, *gn.* N. 3° W. to N. 17° E., 65°-70° W.; 2 miles to 2½ E. of Bedford village, thin to thick *gn.* N. 42° N., 45°-50° W.

AREA 25.—East of N.E. end of area, *m. gn.* or *m. sch.* N. 21° E., 50° W.; ½ mile S.W., thick-bedded *gn.*, porphyritic with some thin micaceous layers, feldspar crystals ¾-1½ inches long, N. 24° E., 55° W., changes to reddish granite; *l.* outcrops to W., but bedding not distinct; 1 mile S.W. of last, thin *gn.*, N. 24° E., var. to N. 42° E., dip W.; ½ mile W. of W. end of area, thick-bedded *gn.* N. 57° E., 35°-40° W.

AREA 26.—1½ mile S.W. of Ridgefield, *m. sch.* N. 37° E., dip 38° W.; 1 mile farther S.W., *l.* N. 3° W. to N. 14° E., 55° W.; 1 mile S.W. of last, and ½ mile W. of *l.*, *gn.* N. 25° E. (average), dip 45°-60° W., again N. 18° E., 55° W.—Near S. end of area, E. of Pound Ridge, W. of lower pond of Trinity Lake, *l.* N. 25°-40° E., 50° W., adjoining *l.* to E., *hbl. sch.* N. 22° E., 55° W.; then E. of this, *l.* N. 12°-22° E., 50°-55° W., and next, rusting *m. sch.* On E. border of valley whitish *gn.* (the mica white), N. 47° E., 45° W.; just east of this, white granulyte (some triclinic feldspar in it), dip W.—Near Pound Ridge, W. of *l.* area, *gn.* (thin to thick-bedded) N. 47°-50°-37° E., 45°-50° W.; ¾ mile to S., rusting *m. gn.*, N. 27° E., 60° W.

AREA 27.—At Cruger's, on R.R., S. of station, N. end of cut, *l.* N. 53°-57° E., dip 60° W.; S. end of cut, N. 81° W., 40°-45° E. (or N.), much jointed; N. side of cove S. of *l.*, 225 yds. from R.R., *l.* N. 28° W., 40° E., beyond up the cove, *l.* N. 53°-63° W., 60° E., and near eastern limit of limestone, *l.* N. 68°-75° W., 70°-80° E.; gneiss along road near by and adjoining the bridge N. 68°-78° E., 65°-80° E.; up slope to north, *m. sch.* and *gn.*, N. 82°-72° E., 70°-80° W. (or N.) and *l.* N. 78°-85° W., 70° E. (or N.); on shore, S. of this cove, gray and flesh-colored *gn.* N. 14° E., 68°-80° W.; in eastern part of *l.* area, *l.* N. 73°-78° W., and *m. sch.* adjoining about east and west, dip of both 75°-80° N., greatly contorted so that in most parts strike undeterminable. West of R.R. station, on shore, *l.* and *m. sch.*, N. 66°-80° E., 70°-85° W. (or N.); ½ mile W. of station, *m. sch.* N. 87° E., and farther W., N. 80° W.

AREA 28.—¾ mile N.E. of Verplanck Point W. of Broadway, *l.* and included *m. sch.* N. 15°-20° E.; arenaceous or gneissic *m. sch.*, 300 yds. from upper end of Broadway (at *d*) N. 7° E. to N. 23° W., 70° E.; *l.* at *j.* (on road 550 yds. W. of Church corner) N. 60° W., 70°-80° E.; some arenaceous *gn.* adjoining it, but outcrop small and poor.

AREA 29.—In Canopus Hollow, at mouth of Sprout Brook, near Iron Works, *l.* N. 47°-54° E., 60°-70° E.; adjoining quartzyte N. 47°-55° E., 60° E.; schistose band in quartzyte to south half way from the point to R.R. station, N. 47°-55°, 60°-75° E.

400 yds. S. of Annsville, on river, *l.* contorted, very fine-grained, slightly crystalline, N. 20°-44° E., N. 42° E. average, 55°-60° E.; E. side of Sprout Brook, near junction with Peekskill Creek, *hydromica sch.* N. 24°-31° E., 60° E.; up brook, at quarry, *l.* N. 32° E. to N. 8° W., 70° E.; above crossing of brook by road, *l.* N. 42°-52° E., dip W., and nearly 150 yds. N.E. of road, *l.* twisted in with *gn.*, N. 8° W. to N. 62° E., and a *gneissiod quartzyte* in the N. side of the hill, N. 67° E. (this is where the T-symbols make an X on the map, and here N. side of valley is bounded by the Highland Archæan about 100 rods distant); just S. of

boundary of county, E. of brook, *l.* N. 27°–32° E., 65°–70° E.; just N. of boundary in Putnam Co.; valley ½ mile wide, *l.* N. 27°–29° E., 70°–75° W.; farther N.E., under bridge (at Continental Village), *l.* (with beds of *quartzyte*) N. 45° E., partly graphitic; bordering *l.* on W., *slate* N. 53° E., 70° E.; ½ mile N.E. of last, *porphyritic granite* (Archæan?); W. of carriage road, the valley nearly ¼ mile wide; ¼ mile N.E. of last, rusting *m. sch.*, N. 17° E., 80° W. to 90°; porph. granite lies northwest of schist; here, on W. side of valley. *l.* N. 37°–44° E., dip W.; *l.* in valley nearly 100 yds. S. of Croft's mine, and thin *m. gn.* west of road; 400 yds. N. of the mine, *l.* impure, N. 25° E., 80° W., same *m. gn.* W. of road; in the valley (Canopus Hollow), between N. end of Solpue Pond and S. end of Oscawana Lake, *l.* N. 53° E., dip E., involved with *quartzytic gneiss*; valley of *l.* here ¼ mile wide, *l.* ends near where the road of the valley crosses the stream here called Canopus Creek.

Area 30A.—See Am. Journ. Sci., xx, 214, 1880, for angles. In Crom Pond street N. of Academy Grounds, Peekskill, thin *m. sch.* N. 85° E., 75°–80° S.; a small show of limestone on the road side, but it may be a loose block.

Area 30.—In Peekskill Hollow at the most N.E. outcrop of *l.* (ib., p. 369), *l.* N. 41°–48° E., 60° W.; also a quarry of *quartzyte* slabs; 100 to 150 ds. of *l.*; at Adams Corners, *l.* white and bluish, very fine grain, N. 47° E., 45°–50° E, but varying much; just below Oregon, *l.* N. 32° E., with *hydromica slate* (looking like argillyte) along side and conformable. N. outcrop of *l.* seen in Peekskill creek valley south of this point.

Area 31.—1½ mile S. of E. of area, hard contorted *gn.*, N. 83° W. to N. 82° E., 80° N.; ½ mile E. of Muscoot River, *l.* nearly E. and W., dip 90°; E. of W. boundary of Somers, near Bennett's, *l.* N. 33° W., N. 8° W., N. 48° W., contorted, dip E.; N. of area, *gn.*, N. 80° E., 80° E. to 90°; ½ mile E. of Hallock's mills, *l.* N. 54° E., 62°–80° E. to 90°.

Area 32.—¾ mile to ½ mile E. of area, *m. gn.* N. 72° E. to N. 88° W., 65° N. to 90°; E. end of area, *l.* N. 74° E., dip 70° N.; near W. end, *l.* N. 72° E.; W. of area, near R.R., hard *gn.* N. 72° E.

Area 34*a*.—S. part of area, *l.* nearly E. and W. to N. 57° E., dip N.; N.W. part, *l.* N. 78° W. to E. and W., 70° W.; *gn.* just N. and ⅛ mile S., conformable. At Golden Bridge, 300 yds. W. of station, *m. sch.*, N. 73° W. to N. 62° E., large granite vein in it; same, *m. sch.*, ½ mile N. of Golden Bridge.

Area 34*b*.—Area W. of L. Waccabuc; *l.* seen in bowlders, but not in place.

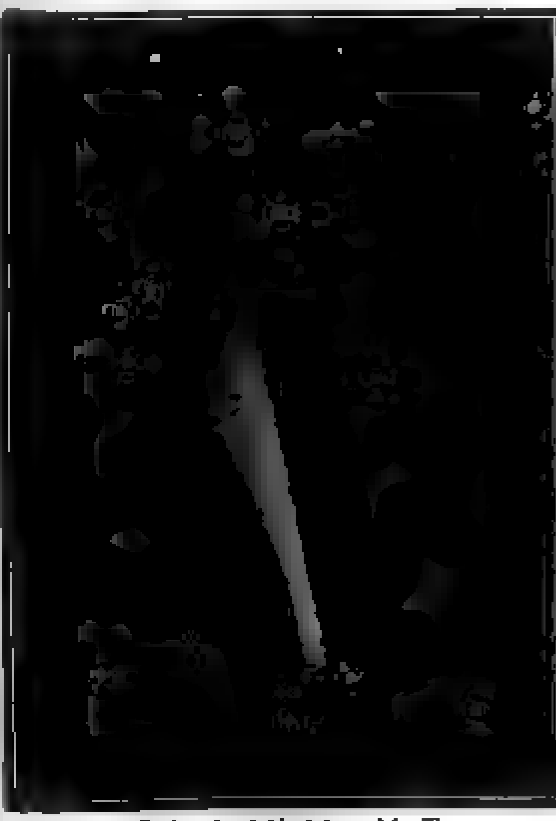
Area 35.—At neck, E. end of lake, N. of brook, *l.* N. 62°–67° E. (var. to N. 57° E.), 50° N.; just N., *m. gn.* N. 62° E., 80° N.; S. of *l.* and end of lake, *m. gn.*, N. 73° W., dip N.; nearly 1 mile E., thin *gn.*, N. 58° E.; but farther west, S. of lake, *gn.* granitoid.

Area 36.—To E. at Connecticut boundary, where the valley is very narrow (and the *l.* may be for a while interrupted), *gn.* N. 60°–62° E., 90° to 80° W.; ⅜ mile E. of N. Salem, *l.* N. 68° W. to N. 82° E., dip N., cryst. very coarse; ⅜ mile S.W. of N. Salem, *l.* N. 67° E., 50°–60° W.; Salem Center, W. of cross roads, hard *gn.* (in the *l.* area) N. 57° E.; 300 yds. W. of S. Center, *l.* N. 77° E., 57° W.; 1 mile W. of Salem Center, near N. margin of *l.*, *gn.* N. 69° W.; S. of last, *l.* N. 78° W., 90°; 400 yds. to W., *l.* N. 66°–73° W., ⅛ mile; ⅙ mile W. of Decker's, *l.* N. 74°–88° W., dip 90°, and ½ mile W. of Decker's, *l.* N. 88° W., 75° N.; N. of *l.* area, N. 88° E., 60° N.; near limekiln W. of Mrs. Bailey's, *l.* N. 87° E.; and just N., *gn.* same dip 80° N.; ⅜ mile W., valley narrows, and *l.* ends.

Area 37.—S. of E. end, thin *gn.* N. 88° W., 70° N.; no *l.* seen where examined, but features those of a *l.* valley. About Peach Lake, mostly hard whitish to gray gneiss, some slightly reddish; at south end strike N. 58°–73° W., dip 55°–60° E. At Croton Falls, near R.R. station, black *micaceous* rock (*hblc.*) N. 73° W., dip to north.

June 26, 13^h 30^m, D. O. M. T.

June 28, 13^h, M. T.



July 1, 12^h 15^m, M. T.

July 22, 14^h M. T.

THE

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[THIRD SERIES.]

ART. XLV.—*Jurassic Birds and their Allies*; by Professor O. C. MARSH.

[Read before Section D., British Association for the Advancement of Science, at York, Sept. 2d, 1881.]

ABOUT twenty years ago, two fossil animals of great interest were found in the lithographic slates of Bavaria. One was the skeleton of *Archæopteryx*, now in the British Museum, and the other was the *Compsognathus* preserved in the Royal Museum at Munich. A single feather, to which the name *Archæopteryx* was first applied by Von Meyer, had previously been discovered at the same locality. More recently, another skeleton has been brought to light in the same beds, and is now in the Museum of Berlin. These three specimens of *Archæopteryx* are the only remains of this genus known, while of *Compsognathus* the original skeleton is, up to the present time, the only representative.

When these two animals were first discovered, they were both considered to be reptiles by Wagner, who described *Compsognathus*, and this view has been held by various authors down to the present time. The best authorities, however, now agree with Owen that *Archæopteryx* is a bird, and that *Compsognathus*, as Gegenbaur and Huxley have shown, is a Dinosaurian reptile.

Having been engaged for several years in the investigation of American Mesozoic birds, it became important for me to study the European forms, and I have recently examined with

some care the three known specimens of *Archæopteryx.* I have also studied in the Continental Museums various fossil reptiles, including *Compsognathus,* which promised to throw light on the early forms of birds.

During my investigation of *Archæopteryx,* I observed several characters of importance not previously determined, and I have thought it might be appropriate to present them here. The more important of these characters are as follows:—

1. The presence of true teeth, in position, in the skull.
2. Vertebræ biconcave.
3. A well-ossified, broad sternum.
4. Three digits only in the manus, all with claws.
5. Pelvic bones separate.
6. The distal end of fibula in front of tibia.
7. Metatarsals separate, or imperfectly united.

These characters, taken in connection with the free metacarpals, and long tail, previously described, show clearly that we have in *Archæopteryx* a most remarkable form, which, if a bird, as I believe, is certainly the most reptilian of birds.

If now we examine these various characters in detail, their importance will be apparent.

The teeth actually in position in the skull appear to be in the premaxillary, as they are below or in front of the nasal aperture. The form of the teeth, both crown and root, is very similar to the teeth of *Hesperornis.* The fact that some teeth are scattered about near the jaw would suggest that they were implanted in a groove. No teeth are known from the lower jaw, but they were probably present.

The presacral vertebræ are all, or nearly all, biconcave, resembling those of *Ichthyornis* in general form, but without the large lateral foramina. There appear to be twenty-one presacral vertebræ, and the same, or nearly the same, number of caudals. The sacral vertebræ are fewer in number than in any known bird, those united together not exceeding five, and probably less.

The scapular arch strongly resembles that of modern birds. The articulation of the scapula and coracoid, and the latter with the sternum is characteristic; and the furculum is distinctly avian. The sternum is a single broad plate, well ossified. It probably supported a keel, but this is not exposed in the known specimens.

In the wing itself the main interest centers in the manus and its free metacarpals. In form and position these three bones are just what may be seen in some young birds of to-day. This is an important point, as it has been claimed that the hand of *Archæopteryx* is not at all avian, but reptilian. The

bones of the reptile are indeed there, but they have already received the stamp of the bird.

One of the most interesting points determined during my investigation of *Archæopteryx* was the separate condition of the pelvic bones. In all other known adult birds, recent and extinct, the three pelvic elements, ilium, ischium and pubis, are firmly anchylosed. In young birds these bones are separate, and in all known Dinosaurian reptiles they are also distinct. This point may perhaps be made clearer by referring to the two diagrams before you, which I owe to the kindness of my friend Dr. Woodward, of the British Museum, who also gave me excellent facilities for examining the *Archæopteryx* under his care. In the first diagram we have represented the pelvis of an American Jurassic Dinosaur allied to *Iguanodon*, and here the pelvic bones are distinct. The second diagram is an enlarged view of the pelvis of the *Archæopteryx* in the British Museum, and here too the ilium is seen separate from the ischium and pubis.

In birds the fibula is usually incomplete below, but it may be coössified with the side of the tibia. In the typical Dinosaurs, *Iguanodon*, for example, the fibula at its distal end stands in front of the tibia, and this is exactly its position in *Archæopteryx*, an interesting point not before seen in birds.

The metatarsal bones of *Archæopteryx* show, on the outer face at least, deep grooves between the three elements, which imply that the latter are distinct, or unite late together. The free metacarpal and separate pelvic bones would also suggest distinct metatarsals, although they naturally would be placed closely together, so as to appear connate.

Among other points of interest in *Archæopteryx* may be mentioned the brain-cast, which shows that the brain, although comparatively small, was like that of a bird, and not that of a Dinosaurian reptile. It resembles in form the brain-cast of *Laopteryx*, an American Jurassic bird, which I have recently described. The brain of both these birds appears to have been of a somewhat higher grade than that of *Hesperornis*, but this may have been due to the fact that the latter was an aquatic form, while the Jurassic species were land birds.

As the *Dinosauria* are now generally considered the nearest allies to birds, it was interesting to find in those investigated many points of resemblance to the latter class. *Compsognathus*, for example, shows in its extremities a striking similarity to *Archæopteryx*. The three clawed digits of the manus correspond closely with those of that genus; although the bones are of different proportions. The hind feet also have essentially the same structure in both. The vertebræ, however, and the pelvic bones of *Compsognathus* differ materially from those of *Archæ-*

opteryx, and the two forms are in reality widely separated. While examining the *Compsognathus* skeleton, I detected in the abdominal cavity the remains of a small reptile which had not been previously observed. The size and position of this inclosed skeleton would imply that it was a fœtus; but it may possibly have been the young of the same species, or an allied form, that had been swallowed. No similar instance is known among the Dinosaurs.

A point of resemblance of some importance between birds and Dinosaurs is the clavicle. All birds have those bones, but they have been considered wanting in Dinosaurs. Two specimens of *Iguanodon*, in the British Museum, however, show that these elements of the pectoral arch were present in that genus, and in a diagram before you one of these bones is represented. Some other *Dinosauria* possess clavicles, but in several families of this subclass, as I regard it, they appear to be wanting.

The nearest approach to birds now known would seem to be in the very small Dinosaurs from the American Jurassic. In some of these, the separate bones of the skeleton cannot be distinguished with certainty from those of Jurassic birds, if the skull is wanting, and even in this part the resemblance is striking. Some of these diminutive Dinosaurs were perhaps arboreal in habit, and the difference between them and the birds that lived with them may have been at first mainly one of feathers, as I have shown in my Memoir on the *Odontornithes*, published during the past year.

It is an interesting fact that all the Jurassic birds known, both from Europe and America, are land birds, while all from the Cretaceous are aquatic forms. The four oldest known birds, moreover, differ more widely from each other than do any two recent birds. These facts show that we may hope for most important discoveries in the future, especially from the Triassic, which has as yet furnished no authentic trace of birds. For the primitive forms of this class we must evidently look to the Paleozoic.

ART. XLVI.—*On the Remarkable Aurora of September* 12–13, 1881; by J. M. SCHAEBERLE.

THE night of Sept. 12–13, 1881, witnessed one of the grandest displays of aurora ever seen in this latitude. Beginning soon after sunset and lasting until the approach of day, the various phenomena which presented themselves during this time are deserving of being placed on permanent record.

The following are some of the notes taken during the night:

7^h 30^m—Ann Arbor, mean time. A grand continuous arch seen spanning the northeastern sky, beginning in the horizon at E. 1° N., and ending N. 45° W.; altitude of highest point of arch 30°; breadth, 5° (close resemblance to cirrus clouds). A second arch, enclosed by the first and 10° from it, quite bright. Space between the arches clear as any part of the sky.

7^h 35^m. At the eastern extremity of the large arc are streamers inclined 70° to the horizon.

7^h 37^m. Four bright streamers in the east, 15° long, $1\frac{1}{2}$° wide, and 2° from each other.

7^h 41^m. Bright streamer, 40° long, $1\frac{1}{2}$° broad, E. 10° N. Bright auroral light in northern horizon; most intense, N. 40° E.

7^h 42^m. Broad sheet of streamers in the east; horizontal motion from east to west, very marked; rate, 1° in 8 seconds of time. Large arch has disappeared.

7^h 50^m. Dark segment; greatest altitude 3°, at N. 20° E. α Aurigæ seen through the same with undiminished luster.

7^h 56^m. Streamers 5° long between N. 30° W., and N. 70° E. It is very evident that the dark segment is nothing but the clear sky, for occasionally a streamer starts from the very horizon and in moving west the dark space seems to offer no resistance.

8^h 2^m. Streamers 15° long; a detached one in Cassiopeia, 45° from the horizon, seen moving westward at the rate of 1° in 3 seconds.

8^h 5^m. Dark segment 5° high; streamers form one continuous sheet of light. In the northeast streamers start 2° from horizon and in their motion westward plough through the dark space.

8^h, 12^m–20^m. Bright arch from N. 30° W. to N. 60° E.; greatest elevation, 10°; occasional streamers, from 3° to 5° in length, shooting from it. Electric lightning in the east.

8^h 31^m. Streamers in the north 15° long; dark segment 6° high, but very irregular in outline.

8^h 33^m. Continuous sheet of light in N.N.E.; no arch.

11^h 45^m. Up to the present time only the auroral twilight could be seen. Moon about three hours high; signs of returning activity; dark segment 7° high.

11^h 53^m. Bright streamer 40° long, N. 10° E.

11^h 54^m. Three arches; whole northern sky covered with streamers 45° in length.

11^h 56^m. Streamers of a reddish tinge, 55° in length; arch broken near the north point; western portion wanting.

$12^h\ 0^m$. Arch 15° high, symmetrical with respect to the meridian. Irregular black patches distributed throughout the space enclosed by the arch; sky in the northwest has a reddish tinge. Motion from east to west, 1° in 3 seconds.

$12^h\ 10^m$. Waves toward the zenith very violent; streamers 50° long.

$12^h\ 13^m$–17^m. Whole northern sky up to 45° altitude, in great commotion; streamers 60° long.

$12^h\ 30^m$–60^m. Streamers from the east and west points of horizon meet south of the zenith, within the square of Pegasus, several parallel spans formed and broken at short intervals.

13^h. Streamers extend 15° southeast of the point of convergence, which is now near α Andromedæ.

$13^h\ 20^m$. Arch from east to west, altitude 25°; vigorous action of auroral waves.

$13^h\ 30^m$. The crossing of the streamers in the zenith gives the appearance of a zigzag motion.

$13^h\ 30^m$. Point of convergence near δ Andromedæ.

$13^h\ 41^m$. Remarkable streamer E. 30° N., beginning in the horizon and for a distance of 8° making an angle of only 30° with it, then suddenly changing its direction to parallelism with the other streamers each side of it which are inclined 75° to the horizon.

$13^h\ 55^m$. Sudden abatement of vigorous action; looks as though the display was coming to a close.

$14^h\ 2^m$. Two arches formed, one in the N.E. the other in the N.N.W., joining each other in the horizon 15° east of north point.

Observations resumed at $15^h\ 45^m$. The view now presented to the observer baffles all description. The whole northern sky from N. 55 W., to N. 55 E. and from the horizon to 60° altitude is one mass of moving fire. The auroral waves succeed each other with great rapidity. Each wave extends throughout the entire width of the aurora, and the flashes toward the zenith are in the form of segments of small circles or zones parallel to the horizon. In the northeast the phenomenon known as the *merry dancers* is very beautiful. A little to the west of north sudden outbursts of light, similar to sheet lightning and having the form of cumulus clouds, instantly appear and disappear at short intervals.

This description can give but a faint idea of the appearance of the aurora at the close of my observations. Athough the moon was still two days from last quarter the phenomena were seen with a vividness truly remarkable. On the following evening the auroral twilight was quite bright until the moon came up. An arch was formed and broken several times. About nine o'clock the northern sky had the appearance of being covered with faint streamers 40° long. Later the aurora gradually died out, and by eleven o'clock no trace of it could be seen.

ART. XLVII.—*Address of Sir John Lubbock, President of the British Association at York.*

[Continued from page 289.]

* * IN Astronomy, the discovery in 1845 of the planet Neptune, made independently and almost simultaneously by Adams and by Le Verrier, was certainly one of the greatest triumphs of mathematical genius. Of the minor planets four only were known in 1831, whilst the number now on the roll amounts to 220. Many astronomers believe in the existence of an intra-mercurial planet or planets, but this is still an open question. The Solar System has also been enriched by the discovery of an inner ring to Saturn, of satellites to Mars, and of additional satellites to Saturn, Uranus and Neptune.

The most unexpected progress, however, in our astronomical knowledge during the past half-century has been due to spectrum analysis.

The dark lines in the spectrum were first seen by Wollaston, who noticed a few of them; but they were independently discovered by Fraunhofer, after whom they are justly named, and who, in 1814, mapped no fewer than 576. The first steps in "spectrum analysis," properly so called, were made by Sir J. Herschel, Fox Talbot, and by Wheatstone, in a paper read before this Association in 1835. The latter showed that the spectrum emitted by the incandescent vapor of metals was formed of bright lines, and that these lines, while, as he then supposed, constant for each metal, differed for different metals. "We have here," he said, "a mode of discriminating metallic bodies more readily than that of chemical examination, and which may hereafter be employed for useful purposes." Nay, not only can bodies thus be more readily discriminated, but, as we now know, the presence of extremely minute portions can be detected, the $\frac{1}{5000000}$th part of a grain being in some cases easily perceptible. It is also easy to see that the presence of any new simple substance might be detected, and in this manner already several new elements have been discovered.

But spectrum analysis has led to even grander and more unexpected triumphs. Fraunhofer himself noticed the coincidence between the double dark line D of the solar spectrum and a double line which he observed in the spectra of ordinary flames, while Stokes pointed out to Sir W. Thompson, who taught it in his lectures, that in both cases these lines were due to the presence of sodium. To Kirchhoff and Bunsen, however, is due the independent conception and the credit of having first systematically investigated the relation which exists between Fraunhofer's lines and the bright lines in the spectra of incan-

descent metals. In order to get some fixed measure by which they might determine and record the lines characterizing any given substance, it occurred to them that they might use for comparison the spectrum of the sun. They accordingly arranged their spectroscope so that one-half of the slit was lighted by the sun, and the other by the luminous gases they proposed to examine. It immediately struck them that the bright lines in the one corresponded with the dark lines in the other—the bright line of sodium, for instance, with the line or rather lines D in the sun's spectrum. The conclusion was obvious. There was sodium in the sun! It must indeed have been a glorious moment when that thought flashed across them, and even by itself well worth all their labor.

But why is the bright line of a sodium flame represented by a black one in the spectrum of the sun? To Ångström is due the theory that a vapor of gas can absorb luminous rays of the same refrangibility only which it emits when highly heated; while Balfour Stewart independently discovered the same law with reference to radiant heat.

This is the basis of Kirchhoff's theory of the origin of Fraunhofer's lines. In the atmosphere of the sun the vapors of various metals are present, each of which would give its characteristic lines, but within this atmospheric envelope is the still more intensely heated nucleus of the sun, which emits a brilliant continuous spectrum, containing rays of all degrees of refrangibility. When the light of this intensely heated nucleus is transmitted through the surrounding atmosphere, the bright lines which would be produced by this atmosphere are seen as dark ones.

Kirchhoff and Bunsen thus proved the existence in the sun of hydrogen, sodium, magnesium, calcium, iron, nickel, chromium, manganese, titanium and cobalt; since which Ångström, Thalen and Lockyer have considerably increased the list.

But it is not merely the chemistry of the heavenly bodies on which light is thrown by the spectroscope; their physical structure and evolutional history are also illuminated by this wonderful instrument of research.

It used to be supposed that the sun was a dark body enveloped in a luminous atmosphere. The reverse now appears to be the truth. The body of the sun, or photosphere, is intensely brilliant; round it lies the solar atmosphere of comparatively cool gases, which cause the dark lines in the spectrum; thirdly, a chromosphere,—a sphere principally of hydrogen, jets of which are said sometimes to reach to a height of 100,000 miles or more, into the outer coating or corona, the nature of which is still very doubtful.

Formerly the red flames which represent the higher regions

of the chromosphere could be seen only on the rare occasions of a total solar eclipse. Janssen and Lockyer, by the application of the spectroscope, have enabled us to study this region of the sun at all times.

It is, moreover, obvious that the powerful engine of investigation afforded us by the spectroscope is by no means confined to the substances which form part of our system. The incandescent body can thus be examined, no matter how great its distance, so long only as the light is strong enough. That this method was theoretically applied to the light of the stars was indeed obvious, but the practical difficulties were very great. Sirius, the brightest of all, is, in round numbers, a hundred millions of millions of miles from us; and, though as big as sixty of our suns, his light when it reaches us after a journey of sixteen years, is at most one two-thousand-millionth part as bright. Nevertheless as long ago as 1815 Fraunhofer recognized the fixed lines in the light of four of the stars, and in 1863 Miller and Huggins in our own country, and Rutherfurd in America, succeeded in determining the dark lines in the spectrum of some of the brighter stars, thus showing that these beautiful and mysterious lights contain many of the material substances with which we are familiar. In Aldebaran, for instance, we may infer the presence of hydrogen, sodium, magnesium, iron, calcium, tellurium, antimony, bismuth, and mercury; some of which are not yet known to occur in the sun. As might have been expected the composition of the stars is not uniform, and it would appear that they may be arranged in a few well marked classes, indicating differences of temperature, or in other words of age. Some recent photographic spectra of stars obtained by Huggins go very far to justify this view.

Thus we can make the stars teach us their own composition with light which started from its source before we were born—light older than our Association itself.

Until 1864, the true nature of the unresolved nebulæ was a matter of doubt. In that year, however, Huggins turned his spectroscope on to a nebula, and made the unexpected discovery that the spectra of some of these bodies are discontinuous—that is to say, consist of bright lines only, indicating that "in place of an incandescent solid or liquid body we must probably regard these objects, or at least their photo-surfaces, as enormous masses of luminous gas or vapor. For it is from matter in a gaseous state only that such light as that of the nebulæ is known to be emitted." So far as observation has yet gone, nebulæ may be divided into two classes: some giving a continuous spectrum, others one consisting of bright lines. These latter all appear to give essentially the same spectrum, consisting of a few bright lines. Two of them, in Mr. Huggins's

opinion, indicate the presence of hydrogen: one of them agrees in position with a line characteristic of nitrogen.

But spectrum analysis has even more than this to tell us. The old methods of observation could determine the movements of the stars so far only as they were transverse to us; they afforded no means of measuring motion either directly towards or away from us. Now Döppler suggested in 1841 that the colors of the stars would assist us in this respect, because they would be affected by their motion to and from the earth, just as a steam-whistle is raised or lowered as it approaches or recedes from us. Everyone has observed that if a train whistles as it passes us, the sound appears to alter at the moment the engine goes by. This arises, of course, not from any change in the whistle itself, but because the number of vibrations which reach the ear in a given time are increased by the speed of the train as it approaches, and diminished as it recedes. So, like the sound, the color would be affected by such a movement; but Döppler's method was practically inapplicable, because the amount of effect on the color would be utterly insensible; and even if it were otherwise the method could not be applied, because, as we did not know the true color of the stars, we have no datum line by which to measure.

A change of refrangibility of light, however, does occur in consequence of relative motion, and Huggins successfully applied the spectroscope to solve the problem. He took in the first place the spectrum of Sirius, and chose a line known as F, which is due to hydrogen. Now, if Sirius was motionless, or rather if it retained a constant distance from the earth, the line F would occupy exactly the same position in the spectrum of Sirius, as in that of the sun. On the contrary if Sirius were approaching or receding from us, this line would be slightly shifted either toward the blue or red end of the spectrum. He found that the line had moved very slightly toward the red, indicating that the distance between us and Sirius is increasing at the rate of about twenty miles a second. So also Betelgeux, Rigel, Castor and Regulus are increasing their distance; while, on the contrary, that of others, as for instance of Vega, Arcturus and Pollux, is diminishing. The results obtained by Huggins on about twenty stars have since been confirmed and extended by Mr. Christie, now Astronomer Royal, in succession to Sir G. Airy, who has long occupied the post with so much honor to himself and advantage to science.

To examine the spectrum of a shooting star would seem even more difficult; yet Alexander Herschel has succeeded in doing so, and finds that their nuclei are incandescent solid bodies: he has recognized the lines of potassium, sodium, lithium and other substances, and considers that the shooting stars are

bodies similar in character and composition to the stony masses which sometimes reach the earth as aërolites.

Some light has also been thrown upon those mysterious visitants, the comets. The researches of Prof. Newton on the periods of meteoroids led to the remarkable discovery by Schiaparelli of the identity of the orbits of some meteor-swarms with those of some comets. The similarity of orbits is too striking to be the result of chance, and shows a true cosmical relation between the bodies. Comets, in fact, are in some cases at any rate groups of meteoric stones. From the spectra of the small comets of 1866 and 1868, Huggins showed that part of their light is emitted by themselves, and reveals the presence of carbon in some form. A photographic spectrum of the comet recently visible, obtained by the same observer, is considered by him to prove that nitrogen, probably in combination with carbon, is also present.

No element has yet been found in any meteorite, which was not previously known as existing in the earth, but the phenomena which they exhibit indicate that they must have been formed under conditions very different from those which prevail on the earth's surface. I may mention, for instance, the peculiar form of crystallized silica, called by Maskelyne, Asmanite; and the whole class of meteorites, consisting of iron generally alloyed with nickel, which Daubrée terms holosiderites. The interesting discovery, however, by Nordenskiöld, in 1870, at Ovifak, of a number of blocks of iron alloyed with nickel and cobalt, in connection with basalts containing disseminated iron, has, in the words of Judd, "afforded a very important link, placing the terrestrial and extra-terrestrial rocks in closer relations with one another."

We have as yet no sufficient evidence to justify a conclusion as to whether any substances exist in the heavenly bodies which do not occur in our earth, thought there are many lines which cannot yet be satisfactorily referred to any terrestrial element. On the other hand, some substances which occur on our earth have not yet been detected in the sun's atmosphere.

Such discoveries as these seemed, not long ago, entirely beyond our hopes. M. Comte, indeed, in his "Cours de Philosophie Positive," as recently as 1842, laid it down as an axiom regarding the heavenly bodies, that "Nous concevons la possibilité de déterminer leurs formes, leurs distances, leurs grandeurs et leurs mouvements, tandis que nous ne saurions jamais étudier par aucun moyen leur composition chimique ou leur structure minéralogique." Yet within a few years what he supposed to be impossible has been actually accomplished, showing how unsafe it is to limit the possibilities of science.

It is hardly necessary to point out that, while the spectrum

has taught us so much, we have still even more to learn. Why should some substances give few, and others many, lines? Why should the same substance give different lines at different temperatures? What are the relations between the lines and the physical or chemical properties.

We may certainly look for much new knowledge of the hidden actions of atoms and molecules from future researches with the spectroscope. It may even, perhaps, teach us to modify our views of the so-called simple substances. Prout long ago, struck by the remarkable fact that nearly all atomic weights are simple multiples of the atomic weight of hydrogen, suggested that hydrogen must be the primordial substance. Brodie's researches also naturally fell in with the supposition that the so-called simple substances are in reality complex, and that their constituents occur separately in the hottest regions of the solar atmosphere. Lockyer considers that his researches lend great probability to this view. The whole subject is one of intense interest, and we may rejoice that it is occupying the attention, not only of such men as Abney, Dewar, Hartley, Liveing, Roscoe and Shuster in our own country, but also of many foreign observers.

When geology so greatly extended our ideas of past time, the continued heat of the sun became a question of greater interest than ever. Helmholtz has shown that, while adopting the nebular hypothesis, we need not assume that the nebulous matter was originally incandescent; but that its present high temperature may be, and probably is, mainly due to gravitation between its parts. It follows that the potential energy of the sun is far from exhausted, and that with continued shrinking it will continue to give out light and heat, with little, if any, diminution for several millions of years.

Like the sands of the sea, the stars of heaven have ever been used as effective symbols of number, and the improvements in our methods of observation have added fresh force to our original impressions. We now know that our earth is but a fraction of one out of at least 75,000,000 worlds.

But this is not all. In addition to the luminous heavenly bodies, we cannot doubt that there are countless others, invisible to us from their greater distance, smaller size, or feebler light; indeed we know that there are many dark bodies which now emit no light or comparatively little. Thus in the case of Procyon, the existence of an invisible body is proved by the movement of the visible star. Again I may refer to the curious phenomena presented by Algol, a bright star in the head of Medusa. This star shines without change for two days and thirteen hours; then, in three hours and a half, dwindles from a star of the second to one of the fourth magnitude; and

then, in another three and a half hours, reassumes its original brilliancy. These changes seem certainly to indicate the presence of an opaque body, which intercepts at regular intervals a part of the light emitted by Algol.

Thus the floor of heaven is not only "thick inlaid with patines of bright gold," but studded also with extinct stars; once probably as brilliant as our own sun, but now dead and cold, as Helmholtz tells us that our sun itself will be, some seventeen millions of years hence.

The connection of Astronomy with the history of our planet has been a subject of speculation and research during a great part of the half century of our existence. Sir Charles Lyell devoted some of the opening chapters of his great work to the subject. Haughton has brought his very original powers to bear on the subject of secular changes in climate, and Croll's contributions to the same subject are of great interest. Last, but not least, I must not omit to make mention of the series of massive memoirs (I am happy to say not yet nearly terminated) by George Darwin on tidal friction, and the influence of tidal action on the evolution of the solar system.

I may perhaps just mention, as regards telescopes, that the largest reflector in 1830 was Sir W. Herschel's of 4 ft., the largest at present being Lord Rosse's of 6 ft.; as regards refractors the largest then had a diameter of 11¼ in., while your fellow townsman Cooke carried the size to 25 in., and Mr. Grubb, of Dublin, has just successfully completed one of 27 in. for the Observatory of Vienna. It is remarkable that the two largest telescopes in the world should both be Irish.

The general result of astronomical researches has been thus eloquently summed up by Proctor:—"The sidereal system is altogether more complicated and more varied in structure than has hitherto been supposed; in the same region of the stellar depths coëxist stars of many orders of real magnitude; all orders of nebulæ, gaseous or stellar planetary, ring-formed, elliptical, and spiral, exist within the limits of the galaxy; and lastly, the whole system is alive with movements, the laws of which may one day be recognized, though at present they appear too complex to be understood."

We can, I think, scarcely claim the establishment of the undulatory theory of light as falling within the last fifty years; for though Brewster, in his "Report on Optics," published in our first volume, treats the question as open, and expresses himself still unconvinced, he was, I believe, almost alone in his preference for the emission theory. The phenomena of interference, in fact, left hardly any—if any—room for doubt, and the subject was finally set at rest by Foucault's celebrated

experiments in 1850. According to the undulatory theory the velocity of light ought to be greater in air than in water, while if the emission theory were correct the reverse would be the case. The velocity of light — 186,000 miles in a second — is, however, so great that, to determine its rate in air, as compared with that in water, might seem almost hopeless. The velocity in air was, nevertheless, determined by Fizeau in 1849, by means of a rapidly revolving wheel. In the following year Foucault, by means of a revolving mirror, demonstrated that the velocity of light is greater in air than in water — thus completing the evidence in favor of the undulatory theory of light.

The idea is now gaining ground, that, as maintained by Clerk-Maxwell, light itself is an electro-magnetic disturbance, the luminiferous ether being the vehicle of both light and electricity.

Wünsch, as long ago as 1792, had clearly shown that the three primary colors were red, green, and violet; but his results attracted little notice, and the general view used to be that there were seven principal colors — red, orange, yellow, green, blue, indigo and violet; four of which — namely orange, green, indigo and violet — were considered to rise from mixtures of the other three. Red, yellow and blue were therefore called the primary colors, and it was supposed that in order to produce white light these three colors must always be present.

Helmholtz, however, again showed, in 1852, that a color to our unaided eyes identical with white, was produced by combining yellow with indigo. At that time yellow was considered to be a simple color, and this, therefore, was regarded as an exception to the general rule, that a combination of three simple colors is required to produce white. Again, it was, and indeed still is, the general impression that a combination of blue and yellow makes green. This, however, is entirely a mistake. Of course we all know that yellow paint and blue paint make green paint: but this results from absorption of light by the semi-transparent solid particles of the pigments, and is not a mere mixture of the colors proceeding unaltered from the yellow and the blue particles: moreover, as can easily be shown by two sheets of colored paper and a piece of window glass, blue and yellow light, when combined, do not give a trace of green, but if pure would produce the effect of white. Green, therefore, is after all not produced by a mixture of blue and yellow. On the other hand, Clerk-Maxwell proved in 1860 that yellow could be produced by a mixture of red and green, which put an end to the pretension of yellow to be considered a primary element of color. From these and other

considerations, it would seem, therefore, that the three primary colors — if such an impression be retained — are red, green, and violet.

The existence of rays beyond the violet, though almost invisible to our eyes, had long been demonstrated by their chemical action. Stokes, however, showed in 1852 that their existence might be proved in another manner, for that there are certain substances which, when excited by them, emit light visible to our eyes. To this phenomenon he gave the name of fluorescence. At the other end of the spectrum, Abney has recently succeeded in photographing a large number of lines in the infra-red portion, the existence of which was first proved by Sir William Herschel.

From the rarity, and in many cases the entire absence, of reference to blue, in ancient literature, Geiger — adopting and extending a suggestion first thrown out by Gladstone — has maintained that, even as recently as the time of Homer, our ancestors were blue-blind. Though for my part I am unable to adopt this view, it is certainly very remarkable that neither the Rig-veda, which consists almost entirely of hymns to heaven, nor the Zendavesta, the Bible of the Parsees or fire-worshippers, nor the Old Testament, nor the Homeric poems, ever allude to the sky as blue.

On the other hand, from the dawn of poetry, the splendors of the morning and evening skies have excited the admiration of mankind. As Ruskin says, in language almost as brilliant as the sky itself, the whole heaven, "from the zenith to the horizon, becomes one molten, mantling sea of color and fire; every black bar turns into massy gold, every ripple and wave into unsullied shadowless crimson, and purple, and scarlet, and colors for which there are no words in language, and no ideas in the mind—things which can only be conceived while they are visible; the intense hollow blue of the upper sky melting through it all, showing here deep, and pure, and lightness; there, modulated by the filmy, formless body of the transparent vapor, till it is lost imperceptibly in its crimson and gold."

But what is the explanation of these gorgeous colors? why is the sky blue? and why are the sunrise and sunset crimson and gold? It may be said that the air is blue, but if so how can the clouds assume their varied tints? Brücke showed that very minute particles suspended in water are blue by reflected light. Tyndall has taught us that the blue of the sky is due to the reflection of the blue rays by the minute particles floating in the atmosphere. Now if from the white light of the sun the blue rays are thus selected, those which are transmitted will be yellow, orange and red. Where the distance is short

the transmitted light will appear yellowish. But as the sun sinks towards the horizon the atmospheric distance increases, and consequently the number of the scattering particles. They weaken in succession the violet, the indigo, the blue, and even disturb the proportions of green. The transmitted light under such circumstances must pass from yellow through orange to red, and thus, while we at noon are admiring the deep blue of the sky, the same rays, robbed of their blue, are elsewhere lighting up the evening sky with all the glories of sunset.

Another remarkable triumph of the last half-century has been the discovery of photography. At the commencement of the century Wedgwood and Davy observed the effect produced by throwing the images of objects on paper or leather prepared with nitrate of silver, but no means were known by which such images could be fixed. This was first effected by Niepce, but his processes were open to objections, which prevented them from coming into general use, and it was not till 1839 that Daguerre invented the process which was justly named after him. Very soon a further improvement was effected by our countryman Talbot. He not only fixed his "Talbotypes" on paper—in itself a great convenience—but, by obtaining a negative, rendered it possible to take off any number of positive, or natural, copies from one original picture. This process is the foundation of all the methods now in use; perhaps the greatest improvements having been the use of glass plates, first proposed by Sir John Herschel; of collodion, suggested by Le Grey, and practically used by Archer; and, more lately, of gelatine, the foundation of the sensitive film now growing into general use in the ordinary dry-plate process. Not only have a great variety of other beautiful processes been invented, but the delicacy of the sensitive film has been immensely increased, with the advantage, among others, of diminishing greatly the time necessary for obtaining a picture so that even an express train going at full speed can now be taken. Indeed, with full sunlight $\frac{1}{600}$ of a second is enough, and in photographing the sun itself $\frac{1}{60000}$ of a second is sufficient.

We owe to Wheatstone the conception that the idea of solidity is derived from the combination of two pictures of the same object in slightly different perspective. This he proved in 1833 by drawing two outlines of some geometrical figure or other simple object, as they would appear to either eye respectively, and then placing them so that they might be seen, one by each eye. The "stereoscope," thus produced, has been greatly popularized by photography.

For 2,000 years the art of lighting had made little if any progress. Until the close of the last century, for instance, our lighthouses contained mere fires of wood or coal, though the

construction had vastly improved. The Eddystone lighthouse, for instance, was built by Smeaton in 1759; but for forty years its light consisted in a row of tallow candles stuck in a hoop. The Argand lamp was the first great improvement, followed by gas, and in 1863 by the electric light.

Just as light was long supposed to be due to the emission of material particles, so heat was regarded as a material, though ethereal, substance, which was added to bodies when their temperature was raised.

Davy's celebrated experiment of melting two pieces of ice by rubbing them against one another in the exhausted receiver of an air-pump had convinced him that the cause of heat was the motion of the invisible particles of bodies, as had been long before suggested by Newton, Boyle and Hooke. Rumford and Young also advocated the same view. Nevertheless, the general opinion, even until the middle of the present century, was that heat was due to the presence of a subtle fluid known as "caloric," a theory which is now entirely abandoned.

Melloni, by the use of the electric pile, vastly increased our knowledge of the phenomena of radiant heat. His researches were confined to the solid and liquid forms of matter. Tyndall studied the gases in this respect, showing that differences greater than those established by Melloni existed between gases and vapors, both as regards the absorption and radiation of heat. He proved, moreover, that the aqueous vapor of our atmosphere, by checking terrestrial radiation, augments the earth's temperature, and he considers that the existence of tropical vegetation—the remains of which now constitute our coal-beds—may have been due to the heat retained by the vapors which at that period were diffused in the earth's atmosphere. Indeed, but for the vapor in our atmosphere, a single night would suffice to destroy the whole vegetation of the temperate regions.

Inspired by a contemplation of Graham Bell's ingenious experiments with intermittent beams on solid bodies, Tyndall took a new and original departure; and regarding the sounds as due to changes of temperature he concluded that the same method would prove applicable to gases. He thus found himself in possession of a new and independent method of procedure. It need perhaps be hardly added that, when submitted to this new test, his former conclusions on the interaction of heat and gaseous matter stood their ground.

The determination of the mechanical equivalent of heat is mainly due to the researches of Mayer and Joule. Mayer, in 1842, pointed out the mechanical equivalent of heat as a fundamental datum to be determined by experiment. Taking the heat produced by the condensation of air as the equivalent of

the work done in compressing the air, he obtained a numerical value of the mechanical equivalent of heat. There was, however, in these experiments, one weak point. The matter operated on did not go through a cycle of changes. He assumed that the production of heat was the only effect of the work done in compressing the air. Joule had the merit of being the first to meet this possible source of error. He ascertained that a weight of 1 lb. would have to fall 772 feet in order to raise the temperature of 1 lb. of water by 1° Fahr. Hirn subsequently attacked the problem from the other side, and showed that if all the heat passing through a steam-engine was turned into work, for every degree Fahr. added to the temperature of a pound of water, enough work could be done to raise a weight of 1 lb. to a height of 772 feet. The general result is that, though we cannot create energy we may help ourselves to any extent from the great storehouse of nature. Wind and water, the coal-bed and the forest, afford man an inexhaustible supply of available energy.

It used to be considered that there was an absolute break between the different states of matter. The continuity of the gaseous, liquid and solid conditions was first demonstrated by Andrews in 1862.

Oxygen and nitrogen have been liquefied independently and at the same time by Cailletet and Raoul Pictet. Cailletet also succeeded in liquefying air, and soon afterwards hydrogen was liquefied by Pictet under a pressure of 650 atmospheres, and a cold of 170° Cent. below zero. It even became partly solidified, and he assures us that it fell on the floor with "the shrill noise of metallic hail." Thus then it was shown experimentally that there are no such things as absolutely permanent gases.

The kinetic theory of gases, now generally accepted, refers the elasticity of gases to a motion of translation of their molecules, and we are assured that in the case of hydrogen at a temperature of 60° Fahr. they move at an average rate of 6,225 feet in a second; while as regards their size, Loschmidt, who had since been confirmed by Stoney and Sir W. Thomson, calculates that each is at most $\frac{1}{50000000}$ of an inch in diameter.

We cannot, it would seem at present, hope for any increase of our knowledge of atoms by any improvement in the microscope. With our present instruments we can perceive lines ruled on glass $\frac{1}{90000}$th of an inch apart. But, owing to the properties of light itself, the fringes due to interference begin to produce confusion at distances of $\frac{1}{74000}$, and in the brightest part of the spectrum at little more than $\frac{1}{90000}$th they would make the obscurity more or less complete. If indeed we could use the blue rays by themselves, their waves being much shorter,

the limit of possible visibility might be extended to $\frac{1}{120000}$; and as Helmholtz has suggested, this perhaps accounts for Stinde having actually been able to obtain a photographic image of lines only $\frac{1}{100000}$th of an inch apart. It would seem then that, owing to the physical characters of light, we can, as Sorby has pointed out, scarcely hope for any great improvement so far as the mere visibility of structure is concerned, though in other respects no doubt much may be hoped for. At the same time, Dallinger and Royston Pigott have shown that, so far as the mere presence of simple objects is concerned, bodies of even smaller dimensions can be perceived.

Sorby is of opinion that in a length of $\frac{1}{80000}$th of an inch there would probably be from 500 to 2,000 molecules—500, for instance, in albumen and 2,000 in water. Even, then, if we could construct microscopes far more powerful than any we now possess, they would not enable us to obtain by direct vision any idea of the ultimate molecules of matter. Sorby calculates that the smallest sphere of organic matter which could be clearly defined with our most powerful microscopes would contain many millions of molecules of albumen and water, and it follows that there may be an almost infinite number of structural characters in organic tissues, which we can at present foresee no mode of examining.

The Science of Meteorology has made great progress; the weather, which was formerly treated as a local phenomenon, being now shown to form part of a vast system of mutually dependent cyclonic and anti-cyclonic movements. The storm-signals issued at our ports are very valuable to sailors, while the small weather-maps, for which we are mainly indebted to Francis Galton, and the forecasts, which anyone can obtain on application either personally or by telegraph at the Meteorological Office, are also of increasing utility.

Electricity in the year 1831 may be considered to have just been ripe for its adaptation to practical purposes; it was but a few years previously, in 1819, that Oersted had discovered the deflective action of the current on the magnetic needle, that Ampère had laid the foundation of electro-dynamics, that Schweizer had devised the electric coil or multiplier, and that Sturgeon had constructed the first electro-magnet. It was in 1831 that Faraday, the prince of pure experimentalists, announced his discoveries of voltaic induction and magneto-electricity, which, with the other three discoveries, constitute the principles of nearly all the telegraph instruments now in use; and in 1834 our knowledge of the nature of the electric current had been much advanced by the interesting experiment of Sir Charles Wheatstone, proving the velocity of the current in a metallic conductor to approach that of the wave of light.

Practical applications of these discoveries were not long in coming to the fore, and the first telegraph line on the Great Western Railway from Paddington to West Drayton was set up in 1838. In America Morse is said to have commenced to develop his recording instrument between the years 1832 and 1837, while Steinheil, in Germany, during the same period was engaged upon his somewhat super-refined ink-recorder, using for the first time the earth for completing the return circuit; whereas in this country Cooke and Wheastone, by adopting the more simple device of the double-needle instrument, were the first to make the electric telegraph a practical institution. Contemporaneously with, or immediately succeeding these pioneers, we find in this country Alexander Bain, Breguet in France, Schilling in Russia, and Werner Siemens in Germany, the latter having first, in 1847, among others, made use of gutta-percha as an insulating medium for electric conductors, and thus cleared the way for subterranean and submarine telegraphy.

Four years later, in 1851, submarine telegraphy became an accomplished fact through the successful establishment of telegraphic communication between Dover and Calais. Submarine lines followed in rapid succession, crossing the English Channel and the German Ocean, threading their way through the Mediterranean, Black and Red Seas, until in 1866, after two abortive attempts, telegraphic communication was successfully established between the Old and New Worlds, beneath the Atlantic Ocean.

In connection with this great enterprise and with many investigations and suggestions of a highly scientific and important character, the name of Sir William Thomson will ever be remembered. The ingenuity displayed in perfecting the means of transmitting intelligence through metallic conductors, with the utmost despatch and certainty as regards the record obtained, between two points hundreds and even thousands of miles apart, is truly surprising. The instruments devised by Morse, Siemens, and Hughes have also proved most useful.

Duplex and quadruplex telegraphy, one of the most striking achievements of modern telegraphy, the result of the labors of several inventors, should not be passed over in silence. It not only serves for the simultaneous communication of telegraphic intelligence in both directions, but renders it possible for four instruments to be worked irrespectively of one another, through one and the same wire connecting two distant places.

Another more recent and perhaps still more wonderful achievement in modern telegraphy is the invention of the telephone and microphone, by means of which the human voice is transmitted through the electric conductor, by mechanism that imposes through its extreme simplicity. In this connection the

names of Reiss, Graham Bell, Edison and Hughes are those chiefly deserving to be recorded.

Whilst electricity has thus furnished us with the means of flashing our thoughts by record or by voice from place to place, its use is now gradually extending for the achievement of such quantitative effects as the production of light, the transmission of mechanical power, and the precipitation of metals. The principle involved in the magneto-electric and dynamo-electric machines, by which these effects are accomplished, may be traced to Faraday's discovery in 1831 of the induced current, but their realization to the labors of Holmes, Siemens, Pacinotti, Gramme, and others. In the electric light, gas-lighting has found a formidable competitor, which appears destined to take its place in public illumination, and in lighting large halls, works, &c., for which purposes it combines brilliancy and freedom from obnoxious products of combustion, with comparative cheapness. The electric light seems also to threaten, when subdivided in the manner recently devised by Edison, Swan, and others, to make inroads into our dwelling-houses.

By the electric transmission of power, we may hope some day to utilize at a distance such natural sources of energy as the Falls of Niagara, and to work our cranes, lifts, and machinery of every description by means of sources of power arranged at convenient centres. To these applications the brothers Siemens have more recently added the propulsion of trains by currents passing through the rails, the fusion in considerable quantities of highly refractory substances, and the use of electric centres of light in horticulture as proposed by Werner and William Siemens. By an essential improvement by Faure of the Planté Secondary Battery, the problem of storing electrical energy appears to have received a practical solution, the real importance of which is clearly proved by Sir W. Thomson's recent investigation of the subject.

It would be difficult to assign the limits to which this development of electrical energy may not be rendered serviceable for the purposes of man. * * *

ART. XLVII.—*The Stereoscope, and Vision by Optic Divergence;* by W. LECONTE STEVENS.

DURING the last twelve years, Professor Joseph LeConte has published in this Journal a series of articles on Binocular Vision, in one of which he refers to a gentleman with normal eyes "who could combine ordinary stereoscopic pictures with the naked eyes beyond the plane of the pictures, even when the distance between the identical points was greater than the distance between the centers of his pupils." He adds, "It would be curious to inquire, at what *distance* and of what *size*, according to the laws of vision, the stereoscopic image ought to seem in this case."*

While conversing with this gentleman,† about three years ago, it was discovered that I possessed the same power; and since that time no stereograph has been found on which identical points were too far apart to secure binocular fusion with the naked eyes. Not until last spring, however, did I begin any careful investigation of these phenomena. Professor LeConte has investigated the phenomena of ocular convergence very fully, and has developed a system of diagrammatic representation far more consistent than any previously published. I have tested all the experiments on this subject that he has described; and my results have been either identical with his, or as closely approximate as could be reasonably expected. To avoid repetition of what has been already sufficiently established I shall assume that the reader is familiar with the contents of Professor LeConte's papers.‡ It will be found convenient to study optic divergence especially in connection with the stereoscope.

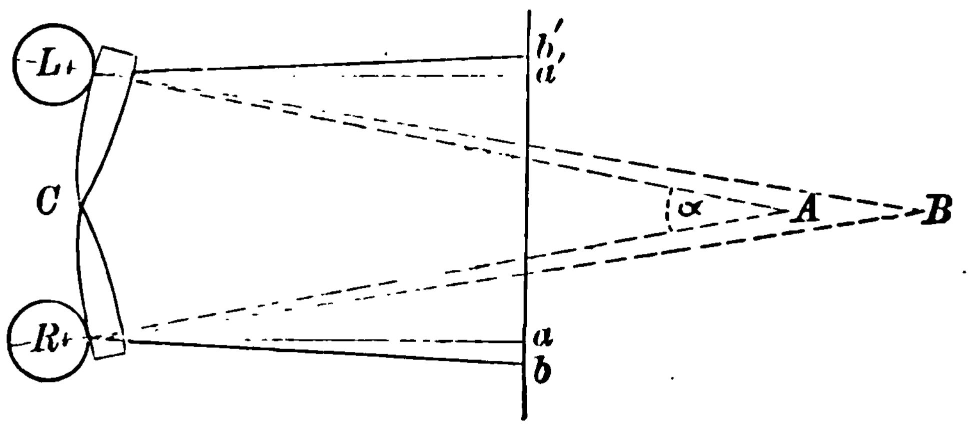
In normal binocular vision the two eyes may be regarded as human cameras occupying slightly different positions, from which are obtained simultaneous views of the point upon which the visual axes are converged. The apparent distance of this point is mainly determined by the intersection of these axes, if the optic angle is large enough to be readily appreciable. In reading ordinary print with comfort the optic angle is rarely less than 12°.

The method of preparing photographs for the stereoscope is too familiar to describe. It is usually assumed that, when these are viewed through the instrument, the lenticular prisms are so adjusted that rays are deviated into the observer's eyes from corresponding points of the stereograph, as if coming from single objects in front; so that he may easily imagine his own

* III, ix, 162–163, March, 1875. † Mr. James Wood Davidson, of New York.
‡ This Journal, II, vol. xlvii, pp. 68 and 153; III, vol. i, p. 33; vol. ii, pp. 1, 315, and 417; vol. ix, p. 159.

eyes to replace the photographer's cameras, and the convergence of his visual axes to replace that of axes from some point in the landscape upon which these cameras have been directed. In fig. 1 let aa' be the foreground interval and bb' that for the background on the stereograph; then the foreground appears at A and the background at B.

1.



To determine the apparent distance of A, let i stand for the observer's interocular distance, RL; a for the optic angle, RAL, and D for the apparent distance required. Then, if a and a' be symmetrical,

$$D = \tfrac{1}{2} i \cot \tfrac{1}{2} \alpha.$$

From this equation it is seen that if a be reduced to zero by making the axes parallel, D becomes infinite and there is no intersection. If a be made negative by causing the axes to pass from convergence through parallelism into divergence, D becomes negative and the intersection is behind the observer's head. In either of these cases a physiological impossibility is implied, if we accept the theory that the apparent distance of the combined external image is determined by the intersection of the observer's visual axes. If, therefore, distinct binocular vision is attainable with the axes either parallel or divergent, and any judgment of distance is possible, however faulty it may be, this fact is sufficient to prove that the theory is imperfect, and other elements must be sought for the determination of the judgment of distance in vision through the stereoscope.

In normal binocular vision axial convergence is the most important one of several elements which together determine the apparent distance of the point of sight, provided the real distance of this be near the lower limit of distinct vision. In such cases the formula just deduced is applicable with little or no modification. If i stand for the distance between two photographer's cameras directed to the same point in a landscape, the formula is also applicable to them, provided there be no lack of uniformity in the media through which the rays pass. In normal vision, moreover, both the focal and axial adjustments of the eyes are consensually adapted to the distance of the object regarded, and the deliverances of the muscular sense from the ciliary and rectus muscles conduce to the same judgment of distance. This judgment is the product of the past experience of the individual, and its accuracy must depend largely upon his acquired skill in interpreting muscular sensations, compar-

ing external relations, and remembering the results of such comparisons. If by any means the axial adjustment can be made to differ considerably from that which usually accompanies a given focal adjustment, binocular vision is to that extent abnormal, and the resulting judgment of distance is correspondingly vitiated. It will be shown that vision through the stereoscope is in nearly all cases abnormal, and that optic divergence is not uncommon among those who use this instrument, especially among young persons whose interocular distance is small, whose eyes are normal, and whose power of accommodation, both focal and axial, is hence large.

If an observer, who possesses but a single eye, looks out upon a landscape, the relative distance of the different objects viewed may be roughly estimated in terms of some standard arbitrarily chosen, so long as they are not precisely aligned with his eye. The judgment is less accurate as the angular separation of the objects becomes less, and as there are fewer of them at moderate distances with which to compare the rest. Always, and often unconsciously, he employs one or more of the following elements in judging the distance and form of each object regarded.

I. Near objects subtend larger visual angles than remote objects of equal size.

II. Near objects are seen more distinctly than those that are remote. The illusion of distance may hence be produced by decreasing the brightness of the object viewed, by changing the nature of the medium, or by increasing the contrast between light and shade.

III. Near objects, that are almost aligned with those which are remote, partly cover them. Covering objects are judged nearer than those covered.

IV. Familiarity with the dimensions of known objects when near enables us to compare them when remote and thereby judge their relative distance.

V. By moving from one standpoint to another and comparing the new view with what is retained in memory of the previous one, parallax of motion thus contributes to the formation of a judgment of both distance and form.

The mere synopsis of these elements is all that is necessary; separately they are familiar enough, and to illustrate them would be easy. Every one of them may be employed in the use of each eye, either separately or in conjunction with its companion. For distances of more than 240^m the binocular observer has no advantage except that two eyes receive more light than one, and the combined external image hence appears brighter and more distinct. All of them except the last may be imitated in pictures, and some of them, notably the second, may be heightened by the magnifying effect of lenses. In study-

ing binocular vision they must be eliminated as far as possible; and all except the first may be nearly eliminated by using only skeleton pictures. In ordinary stereographs their combined effect is usually greater than that due to binocular perspective.

If for convenience we apply the term physical perspective to the combined effect of the elements enumerated, then that of focal and of axial adjustment may be called physiological perspective. The latter might be regarded as mathematical if the theory set forth at the beginning of this paper were strictly applicable in all cases. It is well known that focal adjustment does not vary sensibly for distances of more than 6^m, and that its effect is greatest just beyond the near limit of distinct vision, which is also about the average distance at which a stereoscope card is held when regarded. It is also well known that in normal binocular vision, the convergence of axes does not vary sensibly for distances of more than 240^m. In abnormal vision convergence may be diminished until the limit of parallelism is passed; and the judgment of distance continues to be affected by the relaxation of the interior rectus muscles, or contraction of the exterior rectus, or by both, while the focal adjustment is still adapted to the distance of the object in front held as near as convenient. The judgment of distance which results from the conflict of elements produced by this unusual coördination of muscular actions is necessarily by no means mathematical in accuracy.

While the possibility of securing divergence of axes for normal eyes has been long known, no analysis of the visual phenomena in binocular vision by this method has appeared in print, so far as I am aware. Professor LeConte's diagrams show how to determine the apparent direction of the object viewed, but he says,* "there is no point of sight." There is certainly none determined by intersection of visual axes. In reference to images perceived by abnormal vision, Helmholtz says,† "we judge them according to their nearest resemblance; and in forming this judgment we more easily neglect the parts of the sensation which are imperfect than those which are perfectly apprehended." In combining stereoscope pictures by axial divergence, either with or without the instrument, I secure vision so clear that no defect is appreciable at any point however carefully scrutinized; it does not seem necessary then to assume that any parts of the sensation are neglected. The case was very slightly otherwise during my first experiments in divergence. He makes also the following observation, that I translate from the French edition, which is the latest, of his work on Physiological Optics:‡ "When we compare a stereoscopic

* This Journal, III, vol. ix, p. 163.

† Popular Lectures on Scientific Subjects, 1st series, p. 307.

‡ Optic Physiologique, p. 828, edition 1867.

image, observed by divergence of the visual lines, with very remote real objects visible above the stereoscope, such as a remote chain of mountains, the stereoscopic image appears to us much more remote than real objects the most distant." The apparent anomaly of binocular vision without convergence of axes he refers, in this connection, to our "comparing the sensation produced with that which resembles it the most, and which is not distinguishable from it but by feebler convergence, that is, with what very remote objects give us." So far as axial divergence alone is effective, I am unable to sustain Helmholtz's observation; nor is it sustained by those whom I have tested, every one of them giving results closely accordant with my own, care having been taken to prevent any previous knowledge of my object in questioning them. All that is essential is to secure axial divergence and compare the binocular effect with the monocular effect of the same picture, if the original landscape be not present. Before me is a stereograph representing Alpine scenery, which I combine binocularly, with from 2° 17′ to 2° 40′ of divergence, as foreground and background are successively regarded. On closing the left eye, the apparent distance of a remote mountain is not perceptibly diminished; indeed on account of the decreased brightness of the monocular image, the mountain seems slightly farther. To eliminate physical perspective as much as possible, this being always strong in pictures of landscapes, a stereograph is now taken, representing a white marble statue against a dark background; the stereographic interval can be varied at will, the card having been cut in two. Placing this in the stereoscope, the two pictures are drawn apart until 5° of axial divergence is attained, the experiment being made at a window from which an extensive landscape can be seen for the purpose of comparison. By no effort of imagination can I estimate the apparent distance of the statue to be more than 10^{m}. A stereograph representing a skeleton cone is now substituted, but with the same result.

It may be safe to say therefore that if Helmholtz was examining, by axial divergence in the stereoscope, a picture of the same landscape that lay actually before him, the mountains in the picture appeared farther off than those with which they were at once compared by normal vision with both eyes, all the elements of physical perspective being the same in both cases. This is probably what he meant. But his remark is not necessarily or generally applicable when stereograph and landscape are unrelated. Mere divergence of axes is not enough to reverse physical perspective, but may modify it to some extent and introduce special illusions.

[To be continued.]

ART. XLVIII.—*Note on the Electrical Resistance and the Coefficient of Expansion of Incandescent Platinum;* by E. L. NICHOLS, PH.D. (Göttingen).

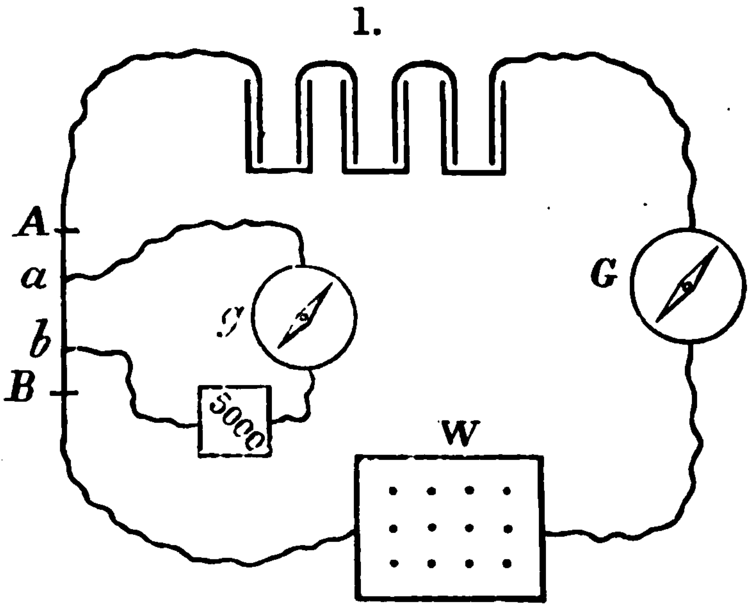
[Read at the Cincinnati Meeting of the American Association for the Advancement of Science, August, 1881.]

I. IN the measurement of temperatures above the red heat, the platinum pyrometer, in one form or another, is as important as the mercury thermometer, at ordinary temperatures. The researches already completed, on the electric resistance and the coefficient of expansion of platinum, and on the specific heat of that metal, only serve, however, to remind us of the much that remains to be done before we may hope to attain to even a fair degree of accuracy in the measurement of temperatures above 500°.

The present writer in order to compare the existing formulæ for the temperature of platinum from its electric-resistance, with those by means of which the temperature is calculated from the coefficient of expansion, and thus to gain a clearer idea of the relative usefulness of the two methods, has determined the resistance and the corresponding length of a platinum wire at various temperatures between 0° and the melting point of that metal.

II. Upon a platinum wire $0{\cdot}4^{mm}$ in diameter and 100^{mm} long, at points 55^{mm} apart and equally distant from the middle of the wire, two very fine platinum wires were welded. They served to mark the ends of the portion of the wire to be measured, and to make electrical connection with a shunt containing a sensitive galvanometer. The wire was heated by the current from a battery of forty Bunsen's cells. Its resistance was determined by the following method.

1.



The wire (AB) (figure 1) together with a tangent galvanometer (G) and a resistance box (W) was in direct circuit with the Bunsen's battery. A very small portion of the current was shunted around *ab*, the portion of the wire to be tested, and carried through a sensitive sine galvanometer (*g*) and through a resistance coil (*w*) of 5000 ohms.

Now with the above arrangement of apparatus, if *w* is very much larger than *r*, the resistance of the wire *ab*, so that the

current through *ab* is not sensibly less than that through the main circuit, we shall have,

$$E=\frac{C}{r}=\frac{C'}{r'},$$

where C and C′ are the currents through *ab* and through the shunt, and r' is the resistance of the shunt.

But

$$C'=\sin U k'$$
$$C=\tan V k$$

where U is the deflection of the sine galvanometer and k' the constant of the instrument, and where V is the deflection of the tangent galvanometer and k the constant of the latter instrument.

Then

$$r=\frac{\tan V}{\sin U}\cdot\frac{k}{k'}=\frac{\tan V}{\sin U}\cdot K$$

where $$K=\frac{k}{k'}r'.$$

The length of the wire *ab* was measured by bringing the two microscopes of a comparator into such position that the terminal (*a*) was in focus in the field of one of the microscopes and (*b*) in the field of the other. Since these points were quite as near the middle as the end of the wire, every change of temperature caused a movement of both (*a*) and (*b*); and it was by taking the differences of these that the true change in the length of *ab* was determined. As the microscopes were provided with excellent micrometer scales and screws, a fair degree of accuracy was obtained by this method. Readings of the length of the wire at 20° agreed with a series taken upon a dividing engine of known accuracy, to within ·002^{mm}. The distance *ab* at 20° was found to be 53·5576^{mm}.

The resistance of the cold wire was found—in terms of U, V and K—by placing the wire in a napthaline bath, and obtaining values of U and V with various amounts of currents. From these readings a curve was drawn with $\frac{\tan V}{\sin U}$ as abscissæ and $\tan^2 V$ as ordinates, $\tan^2 V$ being taken as an expression for the heating effect of the current. The point of this curve corresponding to $\tan^2 V=0$ was taken as the proper value of $\frac{\tan V}{\sin U}$ for the cold wire.

In measuring the resistance of the hot wire, the galvanometers were read simultaneously before and after each determination of the length.

The following table gives the results of the measurements, r temperatures ranging between 0° and a point not far below e melting point of platinum. Both resistance and length of re at 0° are taken equal to unity.

TABLE I.

Resistance.	Length.	Resistance.	Length.
1·0000	1·00000	3·7090	1·01229
1·0410	1·00002	3·7427	1·01223
1·5071	1·00125	3·7813	1·01285
1·9000	1·00289	3·8750	1·01349
2·1212	1·00380	3·8904	1·01371
2·2934	1·00456	3·9305	1·01378
2·3035	1·00489	4·0303	1·01450
2·7821	1·00732	4·0631	1·01469
2·8633	1·00763	4·0655	1·01495
2·9696	1·00809	4·0747	1·01499
3·3533	1·01022	4·0841	1·01514
3·3741	1·01003	4·1248	1·01540
3·4151	1·01042	4·2005	1·01567
3·6449	1·01160	4·2447	1·01632

III. Dr. Siemens has published three formulæ for the varia- on of the resistance of a platinum wire with the temperature. The temperatures were calculated in one case (formula *a*) om the heating effect of a copper ball, the specific heat of pper being regarded as a constant, while the other two rmulæ were derived from measurements with the air-ther- ometer.

These formulæ are:

$$(a)\quad r = \cdot039369\ T^{\frac{1}{2}} + \cdot00216407\ T - \cdot24127$$

$$(b)\quad r = \cdot0021448\ T^{\frac{1}{2}} + \cdot0024187\ T + \cdot30425$$

$$(c)\quad r = \cdot092183\ T^{\frac{1}{2}} + \cdot00007781\ T + \cdot50196$$

here T is the absolute temperature and r the resistance of the ire. The following formula by Benoit is also sometimes sed for the determination of high temperatures.

$$(d)\quad r = 1 + \cdot002445\ t + \cdot000000572\ t^2.$$

this expression t denotes the temperature in degrees centi- ade.

When, as is frequently the case, it is more convenient to easure the length of a wire than its resistance, we may employ atthiesen's formula,

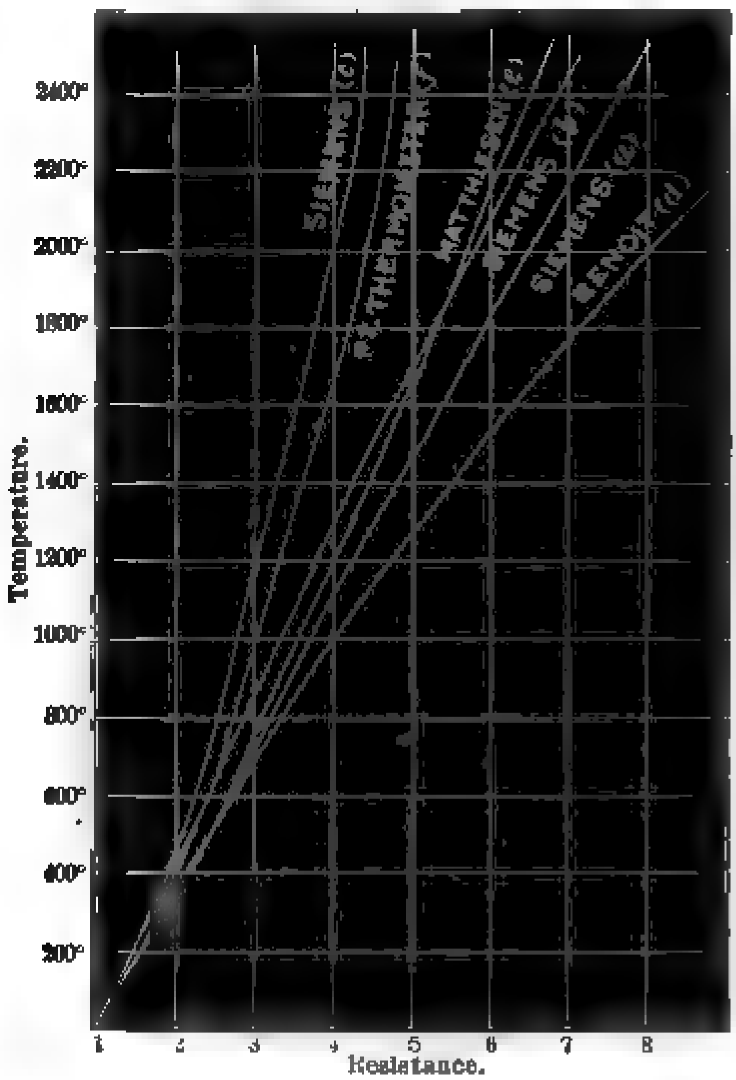
$$(e)\quad l = l_0(1 + \cdot00000851\ t + \cdot0000000035\ t^2)$$

or we may use the uncorrected scale of the platinum thermometer. The latter scale is expressed by the formula

$$(f)\ l = l_o\,(1 + \cdot 00000886\ t).$$

These being almost the only data we possess for the calculation of the temperature of a hot wire, the question of their accuracy is of some importance. The formulæ may be best compared by plotting side by side the curves which represent them (fig. 2).

2.



In fig. 2, resistance is substituted for length in curves (e) and (f), using for that purpose the measurements given in Table I. The following table affords a further comparison of the six formulæ.

In the columns (a) to (f) are given the temperatures, calculated by the several formulæ, at which the resistance of the wire, compared with its resistance at 0°, is given in the column marked "r."

TABLE II.

Length.	r	Siemens. a	b	c	Benoit. d	Matth. e	Pt. thermom. f
1·0000	1·000	0°	0°	0°	0°	0°	0°
1·0032	2·000	325°	402°	420°	378°	342°	375°
1·0082	3·000	692	812	1108	708	726	917
1·0146	4·000	1086	1244	1950	1000	1170	1623
1·0280	5·000	1464	1682	3170	1272	1638	3100
----	6·000	1828	2072	----	1512	2158	----
----	7·000	2170	.2387	----	1766	2800	----
----	8·000	2470	2692	----	1978	----	----
----					----		

A glance at the curves and at this table suffices to show how ill-deserved is the confidence generally felt in these formulæ. The discrepancies involve differences of hundreds of degrees.

IV. The methods employed by Dr. Siemens in the measurements represented by curves *b* and *c* were identical; but the platinum used contained slight impurities. To these impurities the disparity was due. Dr. Siemens found that such foreign substances as usually occur in commercial platinum affected both the resistance of the cold metal and the law of the change of resistance with the temperature.

Benoit's formula (*d*) depends for its accuracy upon the determination of the boiling points of mercury, sulphur, cadmium and zinc; for which temperatures he adopted the values given by Deville and Troost.* M. Ed. Becquerel opposed those values at the time of their publication, and later researches have confirmed him, at least so far as cadmium and zinc are concerned, in thinking them to be entirely too high.

In the following table the results obtained by Deville and Troost are compared with the more probable values given by other physicists.

TABLE III.

Metals.	Boiling points. Dev. and T.	Boiling points. Other values.		
Hg.	360°	350°	Regnault.	
S.	440	448	Bennett,	This Journal, 1878.
		446	Carnelly and	Quart. Jour. Chem. Soc.,
Cd.	860	772	Williams,	1876–78.
Zn.	1040	884	Becquerel,	Comptes Rendus, 57.

The substitution of these values in Benoit's formula, places it more at variance than before with the measurements of Matthiesen and Siemens; a variation probably due to the

* Deville and Troost. Annales de Chimie, III, vol. lviii.

difference of behavior noticed by the latter physicist in the case of different specimens of platinum.

The brief discussion of the above mentioned results suffices we think to show, that:

1st. The formulæ in question are based for the most part upon unwarrantable suppositions, such as the constancy of the specific heat of copper and of platinum; the constancy of the coefficient of expansion of the latter metal, and upon the accuracy of certain very doubtful values for the boiling points of zinc, cadmium, etc.

2d. That, aside from the inaccuracy of those data, the varying resistance of different specimens of platinum renders any formula for the calculation of temperature of that metal from its electric resistance applicable only to the identical wire for which the law of change of resistance with the temperature has been determined.

3d. That from the data at command we are not in position to calculate the temperature of an incandescent platinum wire from its change of resistance, nor from its length, nor indeed in any other manner, further than to express the temperature in terms of the length or the resistance of the wire.

4th. That, owing to the great variations shown by different specimens of platinum as regards its resistance, the determination of the expansion of the wire is to be preferred, whenever practicable, to the measurement of its conductivity.

Art. XLIX.—*On Local Subsidence produced by an Ice-sheet;* by W. J. McGee.*

The influence of a polar ice-cap on the earth's center of gravity has been computed by Croll and others on the supposition of an inflexible crust. But geological investigation has demonstrated that the terrestrial crust is flexible, and hence subject to local deformation. Now the problem requiring the influence of an ice-cap on the earth's center of gravity, on the supposition of a flexible crust, is so complex as to be incapable of solution in the present state of knowledge; but the local deformation may be considered.

The subsidence of areas of deposition is a well-known phenomenon, attested by unequivocal evidence in many parts of the globe. The single instance, cited by Dutton ("Geology of the High Plateaus of Utah," p. 13), of the subsidence of the terrestrial crust in Utah during the Cretaceous-Eocene time to the extent of 6,000 to 15,000 feet, may be here referred to. From

* Supplementary note to p. 267 (line 33) of the last number of this Journal.

this and other instances it appears that a mass of sediment produces a deformation equal to its own thickness. Now since the specific gravity of ice to average rock is something over 1 : 3, it follows that an ice-sheet three miles in thickness ought to depress the subjacent strata about a mile.

But *time* is an important element in the motion of all imperfectly fluid bodies. The approximate numerical equivalence between cause and effect in cases of subsidence with deposition indicates that if sufficient time be given the rigidity of the terrestrial crust is practically *nil;* though it is probable that the function is variable and represented by an infinite series, no terms of which are known. The time of continuance of quaternary ice to that of the deposition of the Cretaceous and Eocene sediments in Utah is as some unknown ratio, probably between 1 : 100 and 1 : 10,000;—say 1 : 1,000. If, however, the deformation during various times is represented by an infinite series, the ratio between quaternary and Cretaceous-Eocene subsidence is much higher—say 1 : 10. The subsidence produced by an ice-sheet three miles in thickness ought accordingly to be only 500 or 600 feet. It will be understood that while it is certain that subsidence would occur, very little value can be attached to this estimate of its amount.

The hydrostatical principles in accordance with which deformation beneath a thick ice-sheet must occur, equally demand that the crust should return to its original form after the melting of the ice; and it is manifest that as much time would be required to produce this secondary as the primary deformation. Assuming then that the periods of advance and retreat, or of growth and decay of the ice are of like duration, it follows that *the earth's surface must continue below the normal level at any latitude, after the withdrawal of the ice, for as long a period as that during which the ice remained stationary at that latitude.*

Should the application of the principles sought to be elucidated in the paper on "Maximum Synchronous Glaciation" to any single continental area ever be attempted, the foregoing considerations will afford a means of testing their accuracy; for late-quaternary depression, being accompanied by submergence in all low-lying areas, has left unmistakable traces, not only of its occurrence but of its extent, in many localities.

Farley, Iowa, Sept. 15, 1881.

ART. L.—*Note on the Laramie Group of Southern New Mexico;* by JOHN J. STEVENSON, Professor of Geology in the University of New York.

IN a former paper* the writer gave some notes respecting the Laramie of Southern New Mexico, as shown in the vicinity of Galisteo creek. Some additional facts respecting the same, obtained during the present summer more than one hundred miles south from Galisteo creek, may be of interest.

The Laramie group is practically continuous on the east side of the Rio Grande Valley, southward from Galisteo creek, to certainly five or six miles beyond San Pedro, or one hundred and fifty miles south from Santa Fe. Coal beds have been opened near Galisteo creek, in the vicinity of the Tuerto mountains, near the Sandia mountains, and at several other localities as far south as San Pedro. The outcrop on the east side of the Rio Grande Valley has been carefully traced and mapped by Mr. J. M. Robinson, for the Atchison, Topeka and Santa Fe railroad company. The absolute continuity of the field is interrupted only by a few narrow cañons and the bluffs marking the western edge of the area can be followed as easily as those marking the eastern edge of the Trinidad coal field in northern New Mexico.

The San Pedro locality is nearly nine miles east from the Rio Grande, and is about twenty-three miles south-southeast from the city of Socorro, whence it can be reached conveniently by a wagon road passing through the villages of San Antonio and San Pedro; but before long it will be more convenient of access, as the railroad company contemplate building a branch road to the coal.

In this southern part of the field one observes the same features as on the Galisteo. Instead of the yellow or buff sandstones which predominate in the Trinidad and Cañon City coal fields, shales prevail, and for the most part the sandstones are soft and often argillaceous. Thin beds of hard, fine-grained sandstone are shown, with distinct jointing and breaking into angular fragments, which retain their sharpness even after long exposure to the weather. When seen from a little distance these thinner beds resemble sheets of igneous rock. As on the Galisteo, beds of iron ore with concretionary structure are numerous, as also are beds of ferruginous clay with cone-in-cone structure. These ferruginous beds are not confined to the lower part of the group. The shales are drab to black and in many of the beds are fissile.

* This Journal, vol. xviii, p. 371.

At the San Pedro locality, four beds of coal were seen within a vertical distance of barely one hundred feet. The lowest bed has the following structure:

Upper division,		
Coal	0′ 8″	
Clay	2′ 6″	
Coal	1′ 4″	
Shale		2′ 3″
Lower division		6′ 9″
Coal	4′ 4″	
Clay	0′ 2″	
Coal	2′ 3″	

The blossom of the next bed at nearly twenty feet higher is somewhat more than five feet thick. The bed contains much coal but it is so broken by partings that perhaps the whole may be unavailable. The third bed is but two or three inches thick and is embedded in dark shale. The highest appears to be little more than two feet thick, the estimate being made from its badly weathered blossom. The dips are southward and vary from seven to fifteen degrees.

The lowest coal bed has been opened by a slope one hundred and fifty feet long, and a large quantity of the coal has been tested on the railroad engines where it worked satisfactorily. Its quality varies in different parts of the bed and the differences in physical characteristics suggest that the relation between fixed carbon and volatile matter may vary in the several benches. The coal from some portions closely resembles semi-anthracite, while that from others cokes readily. This opening is not new, coal having been obtained from it years ago to supply Fort Craig.

These beds belong at not less than two hundred feet above the base of the group.

That this field belongs at the same horizon with the Trinidad coal field has been announced by Mr. Lesquereux, Dr. Hayden and the writer, as proved by the stratigraphy and by the testimony of the fossil plants. In the paper already referred to the writer stated that he had observed on the Galisteo an unexpected intimacy between the Laramie and the Fort Pierre and that he had obtained *Ostrea congesta* from a ferruginous bed high up in the Laramie. This intimacy is much more marked at the San Pedro locality. Stratigraphically and lithologically there is no means of distinguishing the Laramie from the Fort Pierre, aside from the coal beds. Were these absent an observer would hardly hesitate to regard the whole as one group, for there is much less of sandstone here than on the Galisteo. The ferruginous beds with cone-in-cone structure appear to be wholly non-fossiliferous on the Galisteo, but at

the San Pedro locality these beds are fossiliferous, though not to the same extent as the ore-beds. The presence of marine fossils was not ascertained until just before leaving the place, and but a few minutes remained in which to collect. The specimens therefore are such only as could be broken hastily from the weathered surface of the beds, and in most cases suffice for merely generic determination. The list as determined by Prof. R. P. Whitfield is as follows:

Ostrea glabra; Anomia; Corbula, 3 species; *Camptonectes?; Tellina?,* and a fragment of some gasteropod.

ART. LI.—*Polariscopic Observations of Comet c,* 1881; by ARTHUR W. WRIGHT.

THE path of this comet in the sky did not bring it into positions the most favorable for observation, but while near the perihelion it continued for a short time each evening at a sufficient altitude to escape the influence of twilight, though never far enough above the horizon to be viewed under entirely satisfactory conditions. Although these circumstances prevented the attainment of anything like a complete series of observations, it was found possible to establish the fact of polarization, and even to secure some measurements. Owing to the extreme faintness of the light, these were obtained with some difficulty, and were limited to a small number.

The first successful observation was made on August 16, from 9^h to 10^h P. M., local time. With a double-image prism, placed before the eye-piece of a comet-seeker having an aperture of three inches, and a magnifying power of about eight diameters, the light was easily seen to be polarized in a plane passing through the sun. That there might be no doubt upon this point, two other persons were requested to view the images as they appeared in the instrument. Both found one of them fainter in certain positions of the prism, and in every case correctly designated that one which accorded with polarization in the direction above described, and this without any intimation as to the result to be expected. The light was just sufficient, when the polarimeter was applied, to enable the bands to be seen with great difficulty, but measurements were impossible.

A few evenings later some polarimetric determinations were obtained, the results of which are brought together in the annexed table. The instrument and method employed were the same as described in the account of observations upon comet *b.** Column I gives the date and local time; in column

* This Journal, vol. xxii, Aug., 1881, p. 142; Copernicus, No. 8, p. 157; The Observatory, No. 53, p. 253.

II each number is the percentage of polarization derived from ten separate measurements; column III gives the mean of these for each evening; in column IV are given the angles of incidence of the solar rays. These are obtained by graphic interpolation from a curve representing the angles calculated from the ephemeris of H. Oppenheim,* for the dates there given.

I.	II.	III.	IV.
Aug. 20, 8^h 30^m to 9^h 30^m, P. M.	13·4 14·2	13·8	54°·6
Aug. 22, 8^h 30^m to 9^h 15^m, P. M.	11·0 9·7	10·3	55°·6
Aug. 25, 8^h 30^m to 9^h, P. M.	10·5 11·6	11·0	55°·2
Aug. 27, 8^h 30^m to 9^h, P. M.	[16·8]	[16·8]	54°·1

The percentage for August 27 was obtained from two settings of the plates only, and is entitled to less confidence than the others. That the polarization was really increasing, however, was easily recognized by the appearance of the bands, and their relative brightness in the two positions of the glass plates. After this date the condition of the sky was not at any time such as to render further determinations possible. At the hours of the observations the last vestiges of twilight had apparently disappeared; and a careful examination of the neighboring regions of the heavens with the instrument failed to give evidence of its presence, or of any polarization in the very faint light of the sky.

A comparison of the results above given with those obtained in the observations of comet *b*, 1881,† shows that for corresponding angles of incidence the polarization was decidedly less than in the case of the latter comet. There appears also to be a difference in the relation of the percentages to the angles of incidence. Comet *c*, during the period when measurements were possible, changed its position in such a way that the angle first increased and then decreased, the change in each case being very small. It is so little, in fact, that some uncertainty must be felt as to its character, since the data of the published ephemerides lead to considerably different values. That of Oppenheim, however, which was employed in computing the series, as above mentioned, agrees very well with reported observations of position of the comet made during the period covered by the dates in the table. The results found as above indicate that the polarization, for this comet, conforms in general to the law of variation for a gaseous medium, where the curve representing it has the maximum at the incidence of

* Astron. Nachr., No. 2388, p. 190. † Loc. cit.

45°, and changes very rapidly in the region corresponding to the incidences given in the table.

In the case of comet *b*, the largest angle of incidence was nearly 60°, and as this diminished the polarization was seen to diminish likewise; but it happened that at the times of widest incidence the comet was near its perihelion. A maximum occurring with an incident angle as large as 60° would hardly be looked for if the degree of polarization depended upon this angle alone. If the reflecting material were wholly gaseous the greatest polarization should be found at 45° incidence; but though a tendency toward a secondary maximum at this angle may be suspected, the observations are not sufficient to definitely establish its existence. The changes actually observed are with difficulty reconciled with the supposition that the reflection took place from gaseous substance alone. It is not improbable that, as the comet was nearing the sun, and while it remained near the perihelion, some form of volatizable matter may have been eliminated by the increasing temperature, and that the subsequent condensation of this gave rise to the presence of minute liquid or solid particles in the gaseous matter first thrown off. The varying proportions of these two forms of matter might be the cause of notable variations in the total amount of light polarized. It is, of course, not to be overlooked that the substance of the coma, and probably that of the tail, gives out light of itself. The action just described must alter the relation of the emitted to the reflected rays, and this would have its effect upon the degree of polarization.

The earlier observations of comet *b*, made soon after its perihelion passage, show occasional irregularities, and the variations are in some cases decidedly greater than the ordinary errors of observation. The sky at the time appeared very clear, and the atmospheric conditions were probably not the sole cause of the fluctuations. It seems almost certain that at this period of great activity the polarization was subject to considerable variations of an irregular character and comparatively brief duration.

Yale College, October 15, 1881.

ART. LII. — *On the Relative Accuracy of Different Methods of Determining the Solar Parallax*; by WM. HARKNESS.

[The substance of this paper was read before the American Association for the Advancement of Science, at Cincinnati, August, 1881.]

THE object of this paper is to compare the various methods of determining the solar parallax, and to show that the photographic method employed by the United States Transit of Venus Parties in 1874 is among the most accurate known, and should not be neglected in observing the transit of 1882.

The following notation will be employed in algebraic formulæ:

a =mean distance of the earth from the sun.
a_1 =that distance between the earth and the sun which would satisfy Kepler's third law.
a_2 =mean distance of the earth from the moon.
c =a constant such that $c\rho=\rho_1$.
E =the mass of the earth.
e =eccentricity of the moon's orbit.
e_1 =eccentricity of the earth's orbit.
G =observed force of gravity at a point upon the surface of the earth.
k =Gauss's constant for the solar system.
L =constant of the earth's lunar inequality.
l =length of simple pendulum.
M =the mass of the moon.
m =ratio of the mean motions of the sun and moon =0·07480133.
P =the constant of lunar parallax =3422″·7.
P_1=that value of the constant of lunar parallax which would satisfy Kepler's third law.
p =the constant of solar parallax.
Q =the parallactic inequality of the moon.
S =the mass of the sun.
s =geocentric latitude of the moon.
T =length of the sidereal year, expressed in seconds of mean time =31,558,149ˢ.
T_1=length of the sidereal month, expressed in seconds of mean time =2,360,591ˢ·8.
t =time.
V =the velocity of light.
α =the constant of aberration.
γ =Delaunay's constant, which is approximately sin ½ (inclination of lunar orbit to plane of ecliptique), and the exact value of which is 0·04488663. See DTL., vol. ii, 802.
θ =the time taken by light to traverse the mean radius of the earth's orbit.
μ =motion of moon's node, relatively to the line of equinoxes, in 365¼ days.
ν =the heliocentric longitude of the earth.

ν' = the geocentric longitude of the moon.
ρ = the equatorial radius of the earth.
ρ_1 = radius of the earth at latitude φ.
φ = geocentric latitude.
Ψ = the luni-solar precession.
Ω = the constant of nutation.

In citing authorities the following abbreviations will be used:

MAc = Memoires de l'Académie Royale des Sciences. Paris.
HAc = Histoire de l'Académie Royale des Sciences. Paris.
CRH = Comptes Rendus Hebdomadaires des séances de l'Académie des Sciences. Paris.
PTr = Philosophical Transactions of the Royal Society of London.
ANn = Astronomische Nachrichten.
MAS = Memoires of the Royal Astronomical Society. London.
MNt = Monthly Notices of the Royal Astronomical Society, London.
OPM = Annales de l'Observatoire Impérial de Paris. Mémoires.
WOb = Astronomical and Meteorological Observations made at the United States Naval Observatory. Washington.
PTL = Théorie du Mouvement de la Lune, par Jean Plana. Turin, 1832. 3 vols. 4to.
DTL = Théorie du Mouvement de la Lune, par Ch. Delaunay. Paris, 1860–1867. 2 vols. 4to.

Every known method of determining the solar parallax belongs to one or other of the following classes, namely:

I. Trigonometrical methods.
II. Gravitational methods.
III. Photo-tachymetrical methods.

We will consider them in their order.

Trigonometrical Methods.

Observations of Mars, when in opposition to the sun, and at its least distance from the earth, constitute one of the oldest trigonometrical methods of determining the solar parallax. There are two ways of making the observations. Either the planet is observed on or near the meridian, at two stations, situated respectively in the northern and southern hemispheres; or it is observed soon after rising, and just before setting, at a single station. The first method will be termed the meridian method; the second, the diurnal method. In the meridian method the observations may be made either with a transit circle, or with a micrometer attached to an equatorial telescope. In the diurnal method they may be made either with an equatorial telescope, or with a heliometer.

The values of the solar parallax resulting from some of the most noteworthy attempts by the meridian method are as follows:

1672. J. D. Cassini (MAc, viii, 114), ... 9″·5
1751. Lacaille (Ephémérides des Mouvements Célestes depuis 1765 jusqu'en 1774. Paris. Introd. p. 1), 10·38
1835. Henderson (MAS, viii, 103), ... 9·028
1856. Gilliss and Gould (U. S. Ast. Ex. to the South. Hemisphere, vol. iii, p. cclxxxviii), ... 8·495
1863. Winnecke (ANn, Bd. lix, s. 264), ... 8·964
1865. E. J. Stone (MAS, vol. xxxiii, p. 97), ... 8·943
1865. A. Hall (WOb, 1863, App. p. lxiv), ... 8·842
1867. Newcomb (WOb, 1865, App. II, p. 22), ... 8·855
1879. Downing (ANn, Bd. xcvi, s. 127), ... 8·960

The following are some of the results from the diurnal method:

1672. J. D. Cassini (MAc, viii, 107), ... 10″·2
1672. Flamstead (PTr, 1672, p. 5118), ... 10.
1719. Bradley and Pound (Gehler's Physikalisches Wörterbuch, viii, 822), ... 10·5
1857. W. C. Bond (Gould, Ast. Jour., v, 53), ... 8·605
1877. Maxwell Hall (MAS, vol. xliv, p. 121), ... 8·789
1879. Gill (MNt, 1879, vol. xxxix, p. 437), ... 8·78

Owing to the comparative nearness of the asteroids, and their small, well defined disks, it has been thought that the solar parallax might be accurately derived from observations made upon them in the manner just described for Mars. So far as I know, the following are the only attempts which have been made in that direction:

1875. Galle, from Flora (ANn, Bd. lxxxv, s. 267), ... 8″·879
1877. Lindsay and Gill, from Juno (Dunecht Observatory Publications, vol. ii, 211), ... 8·765

The same method has also been applied to Mercury and Venus, but there are great difficulties in the way of obtaining satisfactory results from these planets.

Transits of Venus.—Until quite recently, astronomers have believed that transits of Venus furnish by far the most accurate means of determining the solar parallax. Such transits have been observed by three different methods, namely: 1. By noting the times of contact between the limbs of Venus and the sun. 2. By observing the position of Venus upon the sun's disk with a heliometer. 3. By photographing the sun with Venus upon its disk, and subsequently measuring the photographs.

Contact observations.—The following are some of the results for solar parallax obtained by different astronomers from contact observations of the transits of Venus in 1761, 1769 and 1874:

TRANSIT OF 1761.

1763.	Hornsby (PTr, 1763, p. 494),	9″·73
1763.	Short (PTr, 1763, p. 340),	8·56
1765.	Pingré (HAc, 1765, p. 32),	10·10
1767.	Planman (PTr, 1768, p. 127),	8·49

TRANSIT OF 1769.

1770.	Euler (Novi Commentarii Ac. Sc. Petropol., t. xiv),	8″·8
1771.	Hornsby (PTr, 1771, p. 579),	8·78
1771.	Lalande (HAc, 1771, p. 798),	8·62
·1771.	Maskelyne,	8·723
1772.	Lexell,	8·63
1772.	Pingré (HAc, 1772, p. 419),	8·80
1772.	Planman,	8·43
1814.	Delambre (Astron. Théorique et Pratique, t. i, p. xliv),	8·552
——.	Du Sejour (Traité Analytique des Mouvements Apparent des Corps Celestes, t. i, pp. 451–491),	8·85
1832.	Ferrer (MAS, v, 286),	8·58
1865.	Powalky (Conn. de Temps 1867 Additions, p. 22),	8·832
1868.	E. J. Stone (MNt, vol. xxviii, p. 264),	8·91

TRANSITS OF 1761 AND 1769.

1835.	Encke (Abhand. der Akad. zu Berlin, 1835, Math. Kl., s. 309),	8·571

TRANSIT OF 1874.

1877.	Airy (The Observatory, 1877, vol. i, p. 149),	8·760
1878.	Tupman (MNt, 1878, vol. xxxviii, p. 455),	8·846

The large differences in the parallaxes obtained by different astronomers from the same observations are due to the circumstance that, as the instants of contact are rendered uncertain by the intervention of various disturbing phenomena, many of the observers record two or three different times, corresponding to as many different phases which they endeavor to describe, and thus the resulting parallaxes are influenced to a certain extent by the interpretation put upon these descriptions. The interior contacts give better results than the exterior ones, but in any case the probable error is large. From sixty-one selected observations of interior contacts of the transit of December, 1874, discussed by Col. Tupman (MNt, 1878, vol. xxxviii, 20 on page 450, and 41 on p. 453), I find the probable error of an observed time of contact to be $\pm 4^s\cdot 59$, which corresponds to a probable error of $\pm 0''\cdot 15$ in the distance between the centers of the sun and Venus. Actual errors of from twenty to thirty seconds in the observed times of contacts are by no means uncommon.

Observations with heliometers.—A few heliometers were used in observing the transit of December, 1874, but I am not aware

that anything has yet been published which suffices to show how accurately they will furnish the solar parallax.

Photographic observations.—For observing the last transit of Venus there were used at least two kinds of photoheliographs, constructed upon widely different principles. In what follows I shall consider only the results yielded by apparatus of the kind used by the United States Transit of Venus parties.

As the reductions of the United States transit of Venus observations are not yet quite completed, it is impossible to say exactly what degree of accuracy the photographs will give; but fortunately the same instruments which were used in December, 1874, to observe the transit of Venus at Kerguelen Island, Hobart Town and Peking, were used in May, 1878, to observe the transit of Mercury at Cambridge, Mass., Washington, D. C. and Ann Arbor, Mich.; and as the transit of Mercury photographs are completely reduced, Rear Admiral John Rodgers, Superintendent of the Naval Observatory, has kindly authorized me to make use of the results. They are as follows:

The total number of plates measured was 119, of which 25 were made at Cambridge, 30 at Washington, and 64 at Ann Arbor. Each plate was measured by two different persons. The errors to be considered are of four different kinds, namely: constant and accidental errors in measuring the plates, and constant and accidental errors peculiar to each station.

Each plate having been measured in duplicate, if the positions of Mercury upon the sun's disk given by the measures of the first observer are subtracted from those given by the measures of the second observer, the mean of all the residuals thus obtained will be the constant error due to personal equation in reading. Its amount for each station is

	In altitude.	In azimuth.
Cambridge	$-0''\cdot10$	$-0''\cdot08$
Washington	$-0\cdot09$	$+0\cdot08$
Ann Arbor	$+0\cdot15$	$-0\cdot02$

Thus it appears that, for the mean of the three stations, the constant error of reading is practically zero.

If the mean of the readings by the two observers is accepted as the truth, the probable error of the position of Mercury upon the sun's disk, as determined from a single set of readings by one observer, is

	In altitude.	In azimuth.
Cambridge	$\pm0''\cdot18$	$\pm0''\cdot20$
Washington	$\pm0\cdot19$	$\pm0\cdot18$
Ann Arbor	$\pm0\cdot24$	$\pm0\cdot28$

The locus of the average probable error of reading therefore lies within a circle whose radius is $0''\cdot21$.

The corrections found at each station to LeVerrier's tables of Mercury, as represented by the British Nautical Almanac for 1878, are

	R. A.	N. P. D.
Cambridge	$+0^s{\cdot}079$	$-0''{\cdot}22$
Washington	$+0{\cdot}105$	$-0{\cdot}12$
Ann Arbor	$+0{\cdot}083$	$+0{\cdot}47$

The correction to the north polar distance, given by the Ann Arbor plates, seems to be affected by a systematic error, but it is doubtful if its source can be discovered because no details of the observations were sent to the Naval Observatory, and Professor Watson, who made them, is now dead.

The probable error of a position of Mercury depending upon two sets of readings made upon a single photograph is

	R. A.	N. P. D.
Cambridge	$\pm 0''{\cdot}570$	$\pm 0''{\cdot}562$
Washington	$\pm 0{\cdot}655$	$\pm 0{\cdot}579$
Ann Arbor	$\pm 0{\cdot}436$	$\pm 0{\cdot}514$

The probable errors in right ascension having been reduced to arc of a great circle. We may infer from the mean of all the stations that the average locus of the probable error of the position of the planet in the heavens is a circle whose radius is $0''{\cdot}553$.

To exhibit yet more clearly the degree of accuracy attained by the photographic method, a table is appended, which includes all the plates, and shows the number of residuals, both in right ascension and north polar distance, which fall between $0''{\cdot}0$ and just under $0''{\cdot}2$, $0''{\cdot}2$ and just under $0''{\cdot}5$, etc. In tabulating the right ascension residuals it has been assumed that $0''2 = 0^s{\cdot}01$, $0''{\cdot}5 = 0^s{\cdot}03$, $1''{\cdot}0 = 0^s{\cdot}07$, $1''{\cdot}5 = 0^s{\cdot}10$, $2''{\cdot}0 = 0^s{\cdot}13$.

Limits.	Cambridge.		Washington.		Ann Arbor.	
	R. A.	N. P. D.	R. A.	N. P. D.	R. A.	N. D. P.
$0''{\cdot}0$–$0''{\cdot}2$	3	5	3	7	11	11
0·2–0·5	5	6	5	6	16	14
0·5–1·0	10	7	11	10	29	27
1·0–1·5	5	4	8	3	5	7
1·5–2·0	0	2	2	1	3	5
2·0 and over	2	1	1	3	0	0

Theory of the Gravitational Methods.

We begin the consideration of the gravitational methods by deriving an expression for the solar parallax in terms of the earth's mass.

If l is the length of a simple pendulum which makes one vibration in t seconds of mean time, the observed force of gravity will be

$$G = \frac{\pi^2 l}{t^2} \tag{1}$$

The attraction of the earth at a point upon its surface in geocentric latitude φ is

$$\frac{k^2 E}{\rho_1^{\,2}} \tag{2}$$

The observed force of gravity is the earth's attractive force diminished by the resolved value of its centrifugal force. At the equator the centrifugal force is $G \div 289{\cdot}24$, while in any other latitude it is $G \cos\varphi \div 289{\cdot}24$; and the resolved part of this force acting in the direction of the vertical is $G \cos^2\varphi \div 289{\cdot}24$. Equating the earth's attraction to the force of gravity augmented by the centrifugal force, we have

$$\frac{k^2 E}{\rho_1^{\,2}} = G\left(1 + \frac{\cos^2\varphi}{289{\cdot}24}\right) \tag{3}$$

Whence, by (1)

$$\frac{k^2}{\pi^2} = \frac{\rho_1^{\,2} l}{t^2 E}\left(1 + \frac{\cos^2\varphi}{289{\cdot}24}\right) \tag{4}$$

If T is the length of the sidereal year, expressed in seconds of mean time, and a_1 is that value of the semi-major axis of the earth's orbit which would satisfy Kepler's third law, we have

$$T^2 = \frac{4\pi^2 a_1^{\,3}}{k^2(S+E)} \tag{5}$$

Le Verrier has shown that $a = 1{\cdot}000141 a_1$, (OPM, ii, 60, and iv, 103). Substituting this value in (5), and transposing

$$\frac{k^2}{\pi^2} = \frac{4a^3}{T^2(S+E)\,(1{\cdot}000141)^3} \tag{6}$$

Eliminating k and π between (4) and (6), and rearranging the terms

$$\frac{S+E}{E} = \frac{4t^2 a^3}{l\,T^2 \rho_1^{\,2}(1{\cdot}000141)^3\left(1 + \frac{\cos^2\varphi}{289{\cdot}24}\right)} \tag{7}$$

Owing to the equatorial bulging of the earth, the points which have $\sqrt{\frac{1}{3}}$ for the sine of their geocentric latitude are the only ones upon the surface of the earth at which a pendulum will vibrate as it would if the whole mass of the earth were concentrated at its center. For that reason we take $\sin^2\varphi = \frac{1}{3}$, and consequently $\cos^2\varphi = \frac{2}{3}$. We also put $\rho_1 = c\rho$, and $a \sin p = \rho$. Substituting these values in (7), it becomes

$$\frac{S+E}{E}=\frac{4t^2\rho}{lT^2c^2\sin^2p(1\cdot000141)^3\left(\frac{434\cdot86}{433\cdot86}\right)} \tag{8}$$

The equation $\sin^2\varphi=\frac{1}{3}$, gives $\varphi=35°\ 15'\ 52''$. Adding to this the angle of the vertical, $10'\ 51''$, the geographical latitude is $35°\ 26'\ 43''$, and the corresponding value of $\log c$ is 9·999515. If we take $t=1^s$, the value of l for latitude $35°\ 26'\ 43''$ is 0·992732 meters.* Substituting these values, together with $T=31,558,149$ seconds of mean solar time, and $\rho=6,378,390$ meters, in equation (8), it becomes

$$p^3\left(\frac{S+E}{E}\right)=226,350,000 \tag{9}$$

or

$$p=609\cdot434\sqrt[3]{\frac{E}{S+E}} \tag{10}$$

where p is expressed in seconds of arc.

In connection with equations (9) and (10) the reader may compare "Hansen on the calculation of the sun's parallax from the lunar theory," MNt, 1864, vol. xxiv, p. 11; "Darlegung der theoretischen Berechnung der in den Mondtafeln angewandten Störungen, von P. A. Hansen." Zweite Abhandlung, s. 271; "E. J. Stone on the value of the solar parallax, as deduced from the parallactic inequality in the earth's motion." MNt, 1868, vol. xxviii, p. 23; Le Verrier, in the CRH, 1872, t. lxxv, p. 166, and MNt, 1872, vol. xxxii, p. 322.

The equation of the parallactic inequality of the moon's motion, as given by Newcomb from the theories of Plana and Delaunay, is

$$Q=0\cdot24123\frac{1-M}{1+M}\times\frac{p}{\sin P(1-\frac{1}{6}m^2)} \tag{11}$$

Substituting the numerical values of P and m, and transposing, this becomes

$$p=[8\cdot837088]\,Q\frac{1+M}{1-M} \tag{12}$$

from which p can be found when Q and M are known. The quantity within the square brackets is the logarithm of the number which it represents.

In connection with equations (11) and (12) the reader may compare PTL, t. iii, p. 13; DTL, t. ii, p. 847, equation 342; WOb, 1865, Appendix 2, p. 24; MNt, 1880, vol. xl, p. 468.

The lunar equation of the earth's motion is (OPM, iv, 47)

$$\delta v=-\frac{M}{E+M}\times\frac{\sin p'}{\sin P'}\times\cos s'\sin(v'-v) \tag{13}$$

* Everett, Units and Physical Constants, p. 21.

in which p' and P' are the actual values of the solar and lunar parallaxes at the instant for which $\delta\nu$ is required. For any given lunation, $\delta\nu$ will evidently attain its maximum value when $\sin(\nu'-\nu)=1$, that is, when the longitudes of the sun and moon differ by ninety degrees. If now we have an extensive series of observed values of $\delta\nu$, covering many complete revolutions of the moon's node; $\delta\nu$ will have assumed all possible values, the mean of which will be the constant of the lunar inequality; p' will have assumed all possible values, the mean of which will be the constant of solar parallax; and the moon will have had all possible latitudes, the mean of which will be zero. With P' the case will be somewhat different. It is equal to the constant of lunar parallax, plus a series of terms multiplied by factors made up of the mean anomaly of the sun, the mean anomaly of the moon, the mean distance of the moon from its ascending node, and the difference of the mean longitudes of the sun and moon. All these terms, except those involving the difference of the mean longitudes, will assume all possible values and vanish from the mean. The mean of all the values of P' will therefore be, P + terms depending upon the difference of mean longitudes of the sun and moon.* Turning now to the second volume of Delaunay's theory of the moon, we find that the only term of this kind in the lunar parallax is the one numbered (27), upon page 917, and its value is $28''\cdot1788 \cos 2D$. As we have supposed all our observations of $\delta\nu$ to be made when D was 90°, the value of this term will be $-28''\cdot18$, and the mean value of P' will be $P-28''\cdot18 = 3394''\cdot52$. Substituting the mean values thus found in (13), and rearranging the terms, we obtain

$$p = 0\cdot0164564\,L\left(\frac{E+M}{M}\right) \tag{14}$$

In connection with equation (14) the reader may compare, Le Verrier, OPM, iv, 100; Newcomb, WOb, 1855, App. II p. 28; E. J. Stone, MNt, 1868, vol. xxviii, p. 24.

The Moon's Mass.

Before the solar parallax can be obtained from equations (12) and (14), it is necessary to know the moon's mass. Let us consider the different ways of determining it.

The first determination of the moon's mass was made from the tides, by Newton, in 1687. Since then other investigators have employed the same method, but owing to the theoretical and practical difficulties inherent in it, their results have been so discordant as to command very little confidence. Perhaps

* In strictness it should be the difference of the *true* longitudes of the sun and moon.

the most trustworthy result is that by Mr. Wm. Ferrel of the United States Coast Survey, who found the moon's mass from the tides at Brest $\frac{1}{77.14}$, and from the tides at Boston $\frac{1}{78.64}$, the most probable mean being $\frac{1}{77.5}$. (Jour. Frank. Inst., 1871, vol. lxi, p. 366.)

In 1755, D'Alembert determined the moon's mass from the phenomena of precession and nutation, but to do this with extreme accuracy seems a difficult matter. The most recent attempt is by Mr. E. J. Stone (MNt, 1868, vol. xxviii, p. 43), who considers that his equations are accurate to terms of the third order in the lunar theory. With some changes of notation, they are

$$\left.\begin{aligned} \varepsilon &= \frac{Ma^3}{Sa_2^3} \\ \Psi &= A\varkappa + B\varkappa\varepsilon \\ \Omega &= C\varkappa\varepsilon \end{aligned}\right\} \qquad (15)$$

in which

$$\left.\begin{aligned} A &= 1 + \frac{3e_1^2}{2} \\ B &= 1 + \frac{3e^2}{2} - 6\gamma^2 \\ C &= \frac{2\gamma}{\mu}\left(1 + \frac{e^2 3}{2} - \frac{5\gamma^2}{2}\right) \end{aligned}\right\} \qquad (16)$$

Elimiting $\varkappa$ and ε from the equations (15), and introducing the sines of the parallaxes instead of the mean distances, we get

$$M = \frac{\sin^3 p . A\Omega S}{\sin^3 P\,(C\Psi - B\Omega)} \qquad (17)$$

which becomes

$$M = \frac{[2{\cdot}411505]\,A\Omega}{\sin^3 P\,(C\Psi - B\Omega)} \qquad (18)$$

by substituting the value of S $\sin^3 p$ from (9). The number within the square brackets is the logarithm of the quantity which it represents. Ten must be subtracted from its characteristic.

We will take

$$\begin{aligned} \gamma &= 0{\cdot}04488663 \\ e &= 0{\cdot}0548993 \\ e_1 &= 0{\cdot}0167711 \\ \mu &= -19^\circ\,21'\,20'' = -0{\cdot}337818 \text{ of radius.} \\ P &= 3422''{\cdot}7 \end{aligned}$$

The value here given for e is that used by Delaunay (DTL, ii, 802). The value of P is that found from the Greenwich and Cape of Good Hope observations by Breen (MAS, 1864, vol. xxxii, p. 137) and E. J. Stone (MAS, 1866, vol. xxxiv, p. 16). Substituting these values in (16) and (18), the latter equation becomes

$$\frac{1}{M} = 47{\cdot}0243\ \frac{\psi}{\Omega} - 175{\cdot}705 \tag{19}$$

In connection with equations (18) and (19), the reader may compare PTL, t. iii, pp. 25–29; LeVerrier, OPM, t. iv, p. 101; Serret, OPM, v, 324; Newcomb, WOb, 1865, App. II, p. 28.

About 1795 Delambre seems to have determined the moon's mass from the lunar inequality of the earth's motion. This involves the use of equation (14), but as we propose to employ that equation for determining the solar parallax, we cannot avail ourselves of it for the mass of the moon.

There is yet another way of determining the moon's mass; to wit, by comparing the fall of heavy bodies at the surface of the earth with the fall of the moon in its orbit. The resulting equation will be similar to (8), except that for the masses of the sun and earth we must substitute the masses of the earth and moon, and instead of 1·000141 sin p we must employ the particular value of P which satisfies equation (5) when E+M is substituted in it for S+E, and T is taken to be the length of a sidereal revolution of the moon, expressed in seconds of mean time. Designating these special values of T and P by T_1 and P_1, we have

$$\frac{E+M}{M} = \frac{4t^2\rho}{l T_1^2\, c^2 \sin^3 P_1 \left(\frac{434{\cdot}86}{433{\cdot}86}\right)} \tag{20}$$

Of the four methods just described for determining the moon's mass, that depending upon the tides is not sufficiently accurate, and that depending upon the lunar inequality of the earth's motion is not available, for our purpose. There remain only the two methods represented respectively by equations (19) and (20). Let us see what results they give.

As the luni-solar precession increases continually with the time, its value is now known very accurately. I adopt for it the numbers used by Messrs. Newcomb and Stone (WOb, 1865, App. II, p. 28; MNt, 1868, vol. xxviii, p. 43), namely 50″·378. The constant of nutation is much more uncertain. The following are some of the best modern values:

1842. C. A. F. Peters (Num. Con. Nut., p. 37), ... 9″·223
1844. C. A. F. Peters (Mem. Ac. Sc. St. Petersbourg, 7e sér. t. iii, p. 125), ... 9·216

1856. LeVerrier (OPM, t. ii, p. 174),.................. 9·23
1869. E. J. Stone (MAS. vol. xxxvii, p. 249), ... 9·134
1872. Nyrén (Mem. Ac. Sc. St. Petersbourg, 7e sér. t. xix, No. 2),.................................... 9·236

With $\Psi = 50''\cdot378$, formula (19) gives the mass of the moon corresponding to three different values of the nutation constant as follows:

$$\Omega = 9''\cdot230 \quad M = \frac{1}{80\cdot96}$$

$$\Omega = 9''\cdot223 \quad M = \frac{1}{81\cdot15}$$

$$\Omega = 9''\cdot134 \quad M = \frac{1}{83\cdot65}$$

The change in the moon's mass produced by a small change in the constant of nutation is given by the expression

$$d\left(\frac{1}{M}\right) = -28\cdot1\, d\Omega \tag{21}$$

In view of the fact that Peters attributed a probable error of $\pm 0''\cdot0154$ to his most careful determination of the nutation constant, and in view of the subsequent widely differing determination by E. J. Stone, it can scarcely be supposed that the true value of the nutation is known within $\pm 0''\cdot02$. This corresponds to an uncertainty of $\pm 0\cdot56$ in the reciprocal of the moon's mass.

The length of the sidereal month is 2,360,591·8 seconds of mean solar time. Assuming the observed value of the constant of lunar parallax to be $3422''\cdot7$, Plana's theory gives $3419''\cdot62$, and Delaunay's theory $3419''\cdot59$, for the value of P_1. I adopt $3419''\cdot6$. Substituting these values in formula (20), the resulting mass of the moon is $\frac{1}{81\cdot77}$, and the change in the mass produced by a small change in the adopted parallax is given by the expression

$$d\left(\frac{1}{M}\right) = 5\cdot925\, dP \tag{22}$$

The value of the lunar parallax now generally adopted, depends upon the investigations of Messrs. Breen and E. J. Stone. The results of these two gentlemen agree within $0''\cdot01$. The probable error of Mr. Breen's result is not stated, while that of Mr. Stone's is $\pm 0''\cdot049$. Nevertheless, it is not unlikely that the parallax may be one or two-tenths of a second in error. An error of $0''\cdot1$ would produce an error of 0·59 in the reciprocal of the mass.

Probably the moon's mass is about $\frac{1}{81 \cdot 5}$, but it is quite possible that this estimate may be in error by one part in a hundred. The precession-nutation method is considered one of the best for obtaining the moon's mass, but equations (21) and (22) show that neither it, nor the method by the fall of the moon in its orbit, is likely ever to furnish the mass within one part in a thousand. Throughout all his lunar work Hansen adopted a mass of $\frac{1}{80}$, and in what follows I will assume that the true mass lies between the limits $\frac{1}{80}$ and $\frac{1}{83}$.

Parallax from Gravitational Methods.

Mass of the Earth.—In 1872 LeVerrier obtained the mass of the earth from the inequalities in the motions of Venus and Mars, and the secular variations in the elements of their orbits, produced by it; and from the mass thus found he derived the solar parallax by means of an equation similar to (10). (CRH, 1872, t. lxxv, pp. 165–172; MNt, 1872, vol. xxxii, pp. 322–328.) He gave the resulting parallaxes without directly stating the masses, but it is readily seen that his values were as follows:

(A). From the latitudes of Venus at the moments of the transits in 1761 and 1769, earth's mass $= \frac{1}{325{,}165}$.

(B). From a discussion of the meridian observations of Venus in an interval of one hundred and six years, earth's mass $= \frac{1}{324{,}575}$.

(C). From observations of the occultation of ψ^2 Aquarii by Mars, October 1st, 1672, earth's mass $= \frac{1}{323{,}746}$.

Substituting these values in equation (10), the resulting values of the solar parallax are

A.	$8''\cdot862$
B.	$8\cdot868$
C.	$8\cdot875$

Taking the earth's mass as unity, the change in the parallax produced by a change of one thousand units in the mass of the sun is given by the expression

$$dp = 0\cdot00912\, dS \tag{23}$$

It is difficult to estimate the probable error of the above values of the earth's mass, but Tisserand seems to think it

may be sufficient to affect the parallax by $\pm 0''{\cdot}07$. (CRH, 1881, t. xcii, p. 658.) As the secular variations of the elements of the orbits of Venus and Mars increase continually, they will ultimately attain sufficient magnitude to give a very exact value of the earth's mass, and then this method will furnish the solar parallax with the utmost precision.

Parallactic Inequality.—Professor Newcomb found that the value of the parallactic inequality of the moon deduced by Hansen from the Greenwich and Dorpat observations is $126''{\cdot}46$. (WOb, 1865, App. II, p. 23.)

From 2075 Greenwich lunar observations, made between 1848 and 1866, Mr. E. J. Stone found the parallactic inequality to be $125''{\cdot}36 \pm 0''{\cdot}4$; the probable error being estimated. (MNt, 1867, vol. xxvii, p. 271.)

From the Washington lunar observations, made between 1862 and 1865, Professor Newcomb found the parallactic inequality to be $125''{\cdot}46$. (WOb, 1865, App. II, p. 24.)

From an extended discussion of the whole subject, published in the MNt, 1880, vol. xl, pp. 386 to 411, and 441 to 472, Messrs. Campbell and Neison found the observed value of the parallactic inequality to be (p. 467) either $125''{\cdot}64 \pm 0''{\cdot}09$, or $124''{\cdot}64 \pm 0''{\cdot}25$; the difference arising from the admission or non-admission into the lunar theory of a certain hypothetical forty-five year term.

By substituting these values of Q in equation (12) the following values of the solar parallax result:

Moon's Mass.	$\frac{1}{80}$	$\frac{1}{81}$	$\frac{1}{82}$	$\frac{1}{83}$
$Q = 124''{\cdot}64$	$8''{\cdot}782$	$8''{\cdot}780$	$8''{\cdot}778$	$8''{\cdot}776$
125·36	·833	·831	·829	·827
125·46	·839	·837	·835	·833
125·64	·851	·849	·847	·845
126·46	8·910	8·908	8·906	8·904

These parallaxes are but little affected by the assumed mass of the moon, and depend almost entirely upon the observed value of the parallactic inequality, the relation between small changes of p and Q being

$$dp = 0{\cdot}071\, dQ \tag{24}$$

The original observed values of Q are affected by personal equation, irradiation, blurring, and any error which may exist in the adopted semi-diameter of the moon. It is difficult to estimate how thoroughly these quantities are eliminated from the final result, but the remaining uncertainty probably amounts to a considerable fraction of a second.

Lunar Inequality of the Earth.—From observations at Greenwich, Paris and Kœnigsberg, made during the periods stated, LeVerrier found the following values for the lunar equation of the earth: (OPM, iv, 100)

Greenwich	1816–26	L = 6″·45
"	1827–50	6·56
Paris	1804–14	6·61
"	1815–45	6·47
Kœnigsberg	1814–30	6·43

The mean is 6″·50±0″·023.

Professor Newcomb found the following additional values: (WOb, 1865, App. II, pp. 25 and 26)

Greenwich	1851–64	L = 6″·56 ±0″·04
Washington	1861–65	6·51 ±0·07

With these values of L, equation (14) furnishes the following values of the solar parallax:

Moon's Mass.	$\frac{1}{80}$	$\frac{1}{81}$	$\frac{1}{82}$	$\frac{1}{83}$
L = 6″·50	8″·664	8″·770	8″·878	8″·985
6·51	·678	·784	·892	8·999
6·56	8·744	8·851	8·960	9·068

It would seem that the observed value of L should be quite free from systematic errors, because it depends upon observations of the sun which are always made in the same way. The relation subsisting between small changes in the parallax, the mass of the moon, and the earth's lunar inequality, are given by the equation

$$dp = 1{\cdot}36\, dL + 0{\cdot}107\, d\left(\frac{1}{M}\right) \tag{25}$$

It will be difficult to determine the true value of L within ±0″·02, and at present the uncertainty in the reciprocal of the moon's mass is at least ±0·5. With these data the probable error of p comes out ±0″·06.

Photo-tachymetrical Methods.

Theory.—The photo-tachymetrical methods are quite recent, having come into existence about 1850, when Fizeau and Foucault made their inventions for measuring the velocity with which light traverses moderate distances upon the surface of the earth. From the velocity of light thus obtained the solar parallax may be found by two essentially different methods, to wit:

1st. Deriving from the eclipses of Jupiter's satellites the time occupied by light in traversing the mean distance between the

earth and the sun, and combining this with the measured velocity of light, we have

$$\tan p = \frac{\rho}{V\theta} \tag{26}$$

2d. Assuming the ratio of the earth's orbital velocity to the velocity of light to be represented by the constant of aberration, and combining that constant with the measured velocity of light, we have

$$\tan p = \frac{2\,\pi\rho}{TV\tan\alpha\sqrt{1-e^2_1}} \tag{27}$$

If p and V are eliminated between (26) and (27) we get

$$\tan\alpha = \frac{2\,\pi\theta}{T\sqrt{1-e^2_1}}\,; \tag{28}$$

which shows the relation between α and θ.

For the constants in these equations I adopt

ρ = 6378·39 kilometers (Col. Clarke's value).
T = 31,558,149 seconds of mean time.
e_1 = 0·016771

and the equations become

$$p = \frac{[9{\cdot}11914]}{\theta V} \tag{29}$$

$$p = \frac{[7{\cdot}73269]}{\alpha V} \tag{30}$$

$$\theta = [1{\cdot}38644]\alpha \tag{31}$$

the quantities within the square brackets being the logarithms of the numbers which they represent. In connection with equations (26), (27), (28), the reader may consult Cornu, OPM, t. xiii, pp. A 299–A 301.

Velocity of Light.—The following are the principal experimental determinations of the velocity of light between points upon the earth's surface:

		Kilometers.
1849.	Fizeau (CRH, 1849, t. xxix, p. 90),	315,320
1862.	Foucault (CRH, 1862, t. lv, p. 796: Recueil des travaux scientifiques de Léon Foucault, pp. 216–226),	298,000
1874.	Cornu (OPM, xiii, 293),	300,400
1876.	Helmert (ANn, 1876, bd. lxxxvii, s. 126),	299,990
1879.	Michelson (Proc. Amer. Assoc., 1879, pp. 124–160),	299,940
1881.	Young and Forbes (Nature, 1881, vol. xxiv, p. 303),	301,382

Light Equation.—The time taken by light to traverse the mean radius of the earth's orbit is commonly called the light equation, and there are but two determinations of it from the eclipses of Jupiter's satellites, namely:

1792. Delambre, from more than a thousand eclipses of the first satellite (Astronomie par Jerome le Français (la Lande), 3me edition. Paris, 1792, t. i, Tables astronomiques, p. 238. Also, Tables Ecliptiques des Satellites de Jupiter, par M. Delambre. Paris, 1817, p. vii, ---------- $493^s{\cdot}2$

1874. Glasenapp (Investigation of the eclipses of Jupiter's satellites. A dissertation for the degree of master of astronomy, by S. Glasenapp. Published in the Russian language, at St. Petersburg, 1874, p. 131), ---------- 500·84

Glasenapp considered the probable error of his determination to be $\pm 1^s{\cdot}02$.

Aberration.—The following are the principal determinations of the coefficient of aberration:

1728. Bradley (PTr, 1728, p. 655), ---------- $20''{\cdot}25$
1821. Brinkley (PTr, 1821, p. 350), ---------- 20·37
1840. Henderson (MAS, 1840, xi, 248), ---------- 20·41
1843. W. Struve (ANn, 1843, bd. xxi, s. 58), ---------- 20·445
1844. C. A. F. Peters (ANn, 1844, bd. xxii, s. 119), ---------- 20·503
1850. Maclear (MAS, 1851, vol. xx, p. 98), ---------- 20·53
1861. Main (MAS, 1861, vol. xxix, p. 190), ---------- 20·335

Solar Parallax.—The table below exhibits the various values of the solar parallax deducible from the foregoing values of V, θ and a by means of equations (29) and (30). I have rejected Fizeau and Foucault's values of the velocity of light on the ground that they are merely first approximations, the details of which have never been published; and I have made no use of Helmert's rediscussion of Cornu's value. The last column of the table gives the values of a and θ computed by means of equation (31) from the values of θ and a in the first column.

Velocity of Light ----	299,940	300,400	301,382	
Light equa'n				Aberration
$493^s{\cdot}20$	$8''{\cdot}894$	$8''{\cdot}880$	$8''{\cdot}851$	$20''{\cdot}26$
500·84	8·758	8·745	8·716	20·57
Aberration				Light equa'n
$20''{\cdot}25$	$8''{\cdot}897$	$8''{\cdot}883$	$8''{\cdot}854$	$493^s{\cdot}02$
·335	·860	·846	·817	495·09
·37	·844	·831	·802	495·94
·41	·827	·814	·785	496·91
·445	·812	·799	·770	497·76
·503	·787	·773	·745	499·19
20·53	8·775	8·762	8·734	499·84

The relations between small changes in p, θ, a, and V, are given by the equations

$$dp = -0{\cdot}0177\, d\theta - 0{\cdot}0295\, dV \tag{32}$$

$$dp = -0{\cdot}432\ \ da - 0{\cdot}0295\, dV \tag{33}$$

where θ is in seconds of mean time, a in seconds of arc, and V in thousands of kilometers. To determine p with a probable error not exceeding $\pm 0''{\cdot}01$, the probable errors of the other quantities must not exceed the following values, namely: θ, $\pm 0^{s}{\cdot}40$, and V, $\pm$ 240 kilometers; or a, $\pm 0''{\cdot}016$, and V, $\pm$ 240 kilometers. Whatever may be said respecting V, it is quite certain that our present knowledge of θ and a does not approach this degree of accuracy. The probable error of p seems to be at least $\pm 0''{\cdot}05$.

The photo-tachymetric method is embarrassed by serious theoretical difficulties. 1st. As we are ignorant of the optical constitution of inter-planetary space, we have no sure means of passing from the velocity of light at the earth's surface to its velocity in space. 2d. There is no rigorous proof that the constant of aberration gives the exact ratio of the velocity of light to the earth's orbital velocity. 3d. The velocity of light is the velocity of transmission of a single wave, while Fizeau's and Foucault's methods determine the velocity of transmission of a group of waves. Lord Rayleigh has shown that these two things are not necessarily the same. If the ordinary theory of aberration is accepted the velocity of light to which it refers is the velocity of a single wave, while the velocity determined from the eclipses of Jupiter's satellites is that of a group of waves. (Nature, 1881, vol. xxiv, pp. 382 and 460.)

Respecting the theory of aberration the reader may consult, Ann. de Chimie et de Physique, 1818, t. ix, p. 57; Oeuvres completes d'Augustin Fresnel, t. ii, p. 627; Stokes, in L. E. and D. Phil. Mag. 1845, vol. xxvii, p. 9; 1846, vol. xxviii, p. 76; 1846, vol. xxix, p. 6; Klinkerfues, in ANn, 1866, bd. lxvi, s. 337; 1868, bd. lxx, s. 239; 1870, bd. lxxvi, s. 33; Sohncke, ANn, 1867, bd. lxix, s. 209; Hoek, ANn, 1867, bd. lxx, s. 193; Veltmann, ANn, 1870, bd. lxxv, s. 145; Airy, Greenwich Observations, 1871, p. cxix; Proceed. Roy. Soc. 1873, vol. xxi, p. 121; Villarceau, Conn. de Temps, 1878, Additions; Michelson, this Journal, 1881, vol. xxii, p. 120.

Conclusion.

For convenience of reference the limiting values of the solar parallax, found by the various methods described in the foregoing pages, are presented here. It should be remarked, however, that in selecting these values the results of all discussions

made prior to 1857 have been omitted; except in the case of the transit of 1761, and the smaller of the two values from the transit of 1769.

I.—Trigonometrical methods.	
Mars, meridian observations	8″·84 — 8″·96
" diurnal observations	8·60 — 8·79
Asteroids	8·76 — 8·88
Transit of Venus, 1761	8·49 — 10·10
" " 1769	8·55 — 8·91
" " 1874	8·76 — 8·85
II.—Gravitational methods.	
Mass of the earth	8″·87 ± 0″·07
Parallactic Inequality	8·78 — 8·91
Lunar Inequality	8·66 — 9·07
III.—Photo-tachymetrical methods.	
Velocity and Light Equation	8″·72 — 8″·89
Velocity and Aberration	8·73 — 8·90

To obtain a definitive value of the solar parallax, it would now be necessary to form equations of condition embodying the relations between the various elements involved; to weight these equations; and to solve for p by the method of least squares. But what is the use? It is perfectly evident that by adopting suitable weights, almost any value from 8″·8 to 8″·9 could be obtained; and no matter what the result actually was, it would always be open to a suspicion of having been cooked in the weighting. We only know that the parallax seems to lie between 8″·75 and 8″·90, and is probably about 8″·85. Attack the problem as we will, the results cluster around this central value. All the methods give a probable error of about ±0″·06, and no one of them seems to possess decided superiority over the others. We have nearly exhausted the powers of our instruments, and further advance can only be made at the cost of excessive labor.

In the beginning of the eighteenth century the uncertainty of the solar parallax was fully two seconds; now it is only about 0″·15. To narrow it still further, we require a better knowledge of the masses of the earth and moon, of the moon's parallactic inequality, of the lunar equation of the earth, of the constants of nutation and aberration, of the velocity of light, and of the light equation. All these investigations can be carried on at any time, but there are others equally important which can only be prosecuted when the planets come into the requisite positions. Among the latter are observations of Mars when in opposition at its least distance from the earth, and transits of Venus.

In 1874 all astronomers hoped and believed that the transit of Venus which occurred in December of that year would give the solar parallax within $0''\cdot01$. These hopes were doomed to disappointment, and now, when we are approaching the second transit of the pair, there is less enthusiasm than there was eight years ago. Nevertheless, the astronomers of the twentieth century will not hold us guiltless if we neglect in any respect the transit of 1882. Observations of contacts will doubtless be made in abundance, but our efforts should not cease with them. We have seen that the probable error of a contact observation is $\pm0''\cdot15$, that there may always be a doubt as to the phase observed, and that a passing cloud may cause the loss of the transit. On the other hand, the photographic method cannot be defeated by passing clouds, is not liable to any uncertainty of interpretation, seems to be free from systematic errors, and is so accurate that the result from a single negative has a probable error of only $\pm0''\cdot55$. If the sun is visible for so much as fifteen minutes during the whole transit, thirty-two negatives can be taken, and they will give as accurate a result as the observation of both internal contacts. In view of these facts, can it be doubted that the photographic method offers as much accuracy as the contact method, and many more chances of success?

The transit of 1882 will not settle the value of the solar parallax, but it will contribute to that result, directly as a trigonometrical method, and indirectly through the gravitational methods with which the final solution of the problem must rest. As our knowledge of the earth's mass may be made to depend upon quantities which continually increase with the time, it will ultimately attain great exactness, and then the solar parallax will be known with the same exactness. Long before that happy day arrives the present generation of astronomers will have passed over to the silent majority, but not without the satisfaction of knowing that their labors will contribute to that fullness of knowledge which shall be the heritage of their successors.

Washington, D. C., October, 1881.

ART. LIII.—*On the Nature of Cyathophycus*; by C. D. WALCOTT.

THIS genus was originally described by me under the impression that the form was an alga of a peculiar appearance.* On reading the observations of Professor R. P. Whitfield, on the nature of Dictyophyton and its affinities to certain sponges,† it was instantly suggested that Cyathophycus was probably a member of the same group. A special effort was

* Trans. Albany Institute, vol. x, 1879. † This Journal, xxii, July, Aug., 1881.

made to obtain perfectly preserved specimens of the genus, and with such success that the reticulate structure mentioned in the original description was found to be formed of a horizontal and perpendicular series of narrow bands crossing each other at right angles so as to form a network with rectangular interspaces, the narrow bands being formed of thread-like spiculæ resting on, or one against the other. The spiculæ differ in size; some are filiform while others are stronger and more prominent, and all appear to be replaced by pyrite as in the Devonian specimens studied by Principal Dawson and Professor Whitfield. Through the kindness of Professor Whitfield I have had the opportunity of examining the specimens referred to by him, and now have little doubt but that the Utica slate form belongs to the same class, although probably differing generically from the Devonian species, and is an earlier representative of this interesting group of sponges.

Cyathophycus reticulatus presents a beautiful appearance when a large number of specimens are flattened out on a slab of the dark slate. Each individual lays free from its associates and the striking resemblance to *Euplectella* is seen at a glance, although the convex summit of the latter genus is absent and the margin curves over and downward on the inside to a considerable distance at least, how far is yet unknown. The cylindrical forms vary in length from 10 to 350^{mm}, and the spheroidal species, *C. subsphericus*, from 3 to 60^{mm} in diameter, each species preserving the rounded rim of the circular aperture at the summit.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *International Congress of Electricians.* (Letter from Professor G. F. Barker, of the United States Delegation, dated Paris, Oct. 1st, 1881.)—My duties here as Commissioner, as Delegate, and now as a member of the Jury, have been, and still are, so pressing that I have been obliged to forego letter writing almost entirely. I have tried too, to put together some points of interest for the readers of the Journal, but have thus far been quite unable to complete anything.

The Exhibition as a whole has been a decided success. It has brought together an immense mass of highly interesting material. There are in all something over 1500 exhibitors, of which one half are French, 155 Belgian, 115 English, 114 German, 81 Italian, 72 American, 39 Austrian, 32 Russian, 21 Swedish, 13 Swiss, 17 Spanish, 13 Norwegian, 11 Dutch, 5 Danish, and 2 Japanese. Of decided novelties, there are more in the United States section

than in any other. Edison has made a wonderful exhibition of his inventions and his rooms are thronged continually. The principle discovered by him that an electric current varies friction, the so-called motograph principle, together with the applications of it practically, are beautifully illustrated. The principle of the varying resistance of bodies which imperfectly conduct, when they are subjected to pressure, a principle which he was the first to investigate and to apply, is exhibited in a large series of instruments, one set of which traces the progress of development of the carbon telephone. The system of incandescent lighting which he has perfected is shown in all its details, from the unique dynamo-machine of low resistance and high electromotive force, the street conductors with their connections, safety-catches, expansion-caps, etc., the ingenious meter and the house conductors with their incombustible covering, to the fixtures with double conductors and safety catches, and lastly to the incandescent lamp itself. Dolbear exhibits a new electro-static telephone which performs admirably and which consists simply of two thin metal plates, connected to the secondary wire by an induction coil. They are oppositely charged by the coil and so attract each other. Gray's harmonic multiple telegraph is in successful operation and Bell's original photophone is also exhibited. The most original thing exhibited in the French section is the secondary battery; Planté exhibits several forms of it, Faure shows the improvement which he made by covering the plates with minium, and lastly Meritens is working a still newer form, in which only lead plates are used, but a large number of them are put in a small space. In the historical line the collection in the Exhibition is unrivaled. The pile of Volta, the electroscopes of Galvani, the thermopiles of Nobili and Melloni, the electro-magnetic induction ring of Faraday, the first magneto-machine of Pixii, the rheostats and telegraphs of Wheatstone, the telegraphs of Sœmmering, of Steinheil and of Gauss and Weber, the continuous current-machine of Pacimotti, the electro-thermic and electro-motor apparatus of Becquerel, the electro-capillary apparatus of Lippmann; all these and many more are here collected. And as for arc lights, the Exhibition at night is like day. The Brush machine and light are in great favor. A large lamp of this sort just put up has carbons two inches in diameter, and is claimed to give a light of 80,000 candles.

2. *Elasticity and Motion.*—Sir W. THOMSON is led from the consideration of various experiments with fluids and solids and the study of smoke rings to speculate upon elasticity as an evidence of motion. The kinetic theory of gases requires that the molecule or atom shall be elastic. "But this kinetic theory of matter is a dream and must remain so until it can explain chemical affinity — electricity, magnetism, gravitation and inertia." The writer looks forward to a greater generalization which shall include elasticity as a form of motion.—*Roy. Inst. of Great Britain*, March, 1881. J. T.

3. *Efficiency of Spectroscopes.*—F. LIPPICH discusses the point whether it is more advantageous to increase the dispersion or to increase the magnifying power of the telescopes of a spectroscope. A mathematical discussion of the subject is given and the following conclusion is reached: The common impression that it is better to increase the dispersion instead of the magnifying power of the telescope is true only when the number of prisms does not exceed a certain number (from four to five). The author has constructed a spectroscope of two flint glass prisms, through which the light passes twice, provided with a telescope of magnifying power from fifty to seventy times, which excels in its performance that of a spectroscope of from twenty to twenty-eight flint glass prisms which has a telescope which magnifies only ten times. Seven lines are seen with the author's spectroscope between the D lines.—*Central-Zeit. f. Opt. u. Mech.*, 49 and 61, 1881.
J. T.

4. *Niagara Falls as a source of Energy.*—Sir WM. THOMSON thus sums up, in his British Association Address, the conclusions he has reached in regard to the utilization of the energy of Niagara Falls.

"1. Apply dynamos driven by Niagara to produce a difference of potential of 80,000 volts between a good earth connection and the near end of a solid copper wire of half an inch (1·27 centimeters) diameter, and 300 statute miles (483 kilometers) length.

"2. Let resistance by driven dynamos doing work, or by electric light, or, as I can now say, by a Faure battery taking in a charge, be applied to keep the remote end at a potential differing by 64,000 volts from a good earth plate there.

"3. The result will be a current of 240 webers through the wire taking energy from the Niagara end at the rate of 26,250 horse power, losing 5,250 (or 20 per cent) of this by the generation and dissipation of heat through the conductor and 21,000 horse power (or 80 per cent of the whole) on the recipients at the far end.

"4. The elevation of temperature above the surrounding atmosphere, to allow the heat generated in it to escape by radiation and be carried away by connection is only about 20° Centigrade; the wire being hung freely exposed to air like an ordinary telegraph wire supported on posts.

"5. The striking distance between flat metallic surfaces with difference of potentials of 80,000 volts (or 5,000 Daniells') is only eighteen millimeters, and therefore there is no difficulty about the insulation.

"6. The cost of the copper wire, reckoned at 8d. per pound, is 37,000*l.*, the interest on which at five per cent is 1900*l.* a year. If 5,250 horse power at the Niagara end costs more than 1900*l.* a year, it would be better economy to put more copper into the conductor; if less, less."—*Nature, Sept.* 8, 1881, p. 435. J. T.

5. *Change of plane of polarization of Heat rays by Electromagnetism.*—LEO GRUNMACH reviews the work of previous experimenters and arrives at the following conclusions:

(1.) A change of the plane of polarization of the heat rays can be produced in solid and fluid bodies by electromagnetism.

(2.) The magnitude of this change is different for different substances. The rotation is greater the greater the index of refraction of the substance.

(3.) The magnitude of the rotation in diathermanous bodies is proportional to the intensity of the current.

(4.) The magnitude of the rotation in a diathermanous body, placed between the poles of a magnet, is proportional to the magnetic force employed.

(5.) It also increases with the length of the layer of the substance: but this relation can not be computed from the length of the layer.—*Ann. der Physik und Chemie*, No. 9, 1881, p. 85. J. T.

6. *Electro dynamic-Balance.*—H. HELMHOLTZ provides an ordinary balance with two spirals of copper wire, in place of the pans. Beneath these spirals are also two spirals of larger radius. The terminals of these spirals are so arranged that one of the movable spirals is attached and the other repelled. The conditions of sensibility are discussed and the author concludes that the current which is equilibrated by one gram can be measured to $\frac{1}{2000}$ of its value.—*Ann. der Physik und Chemie*, No. 9, 1881, p. 52. J. T.

7. *Change of the thermo-electric condition of iron and steel by magnetization.*—V. STROUHAL and C. BARUS confirm the observation of Sir W. Thomson that a longitudinally magnetized iron wire is thermo-electrically more positive than a non-magnetic iron wire. Their results show that the changes in the thermo-electric condition of iron can not be used to indicate the hardness of the iron or steel. The thermo-electric current between pieces of iron of different magnetic conditions flows in the opposite direction from that which arises between pieces of different degrees of hardness. In other words it flows from the better conductor to the worse conductor.—*Ann. der Physik und Chemie*, No. 9, 1881, p. 54. J. T.

8. *Principles of Chemical Philosophy;* by JOSIAH PARSONS COOKE, Erving Professor of Chemistry and Mineralogy in Harvard College. Revised edition, 623 pp., 8vo. Boston, 1881, (John Allyn).—The first edition of Professor Cooke's valuable work on Chemical Philosophy was published in 1868. The years which have elapsed since then have brought fewer radical changes in the philosophy of chemical phenomena than those which immediately preceded, but the advances which have been made are hardly less important. The new edition is written from this advanced standpoint, and while it contains all the excellent features of the former it embraces also much that is valuable and new. The student who will faithfully read the successive chapters, and together with that, work out the many practical problems, cannot fail to gain a clear, connected and logical knowledge of the fundamental principles in chemical philosophy.

9. *A Manual of Sugar Analysis*, including the applications in general of the analytical methods to the Sugar Industry, with an Introduction on the Chemistry of Cane-sugar, Dextrose, Levulose

and Milk-sugar, by J. H. Tucker, Ph.D. 353 pp. 8vo. New York, 1881 (D. Van Nostrand).—In consideration of the great importance of the sugar industry it is a matter of surprise that up to this time the various topics connected with the analytical portions of the subject have never been systematically discussed in any single volume in the English language. This deficiency the author has aimed to fill. The opening chapters of his work are devoted to the chemistry of the several kinds of sugar. Following these the methods used in the examination of sugars are described; first the determination of the specific gravity, then the optical method of study, and finally the chemical methods. The last are extended over a series of chapters giving the method of analysis of raw sugar, of molasses and syrup, of cane and cane juice, beet and beet juice, of the waste products, of glucose or starch sugar, and so on. The concluding chapters are devoted to the chemistry of animal charcoal. The book contains a large amount of useful information which will be hardly found elsewhere in so convenient a form.

II. Geology and Mineralogy.

1. *Report on the Geology and Resources of the Black Hills of Dakota;* by Henry Newton and Walter P. Jenney. U. S. Geogr. and Geol. Survey of the Rocky Mountain Region, J. W. Powell in charge. 566 pp. 4to, with an atlas folio, 18 plates, in 4to, and many wood-cuts. Washington, 1880. (Copy of the work received in September, 1881.)—The Geological Report, which occupies two hundred pages of this volume, is based on the observations of Mr. Henry Newton, made in 1875, in accordance with instructions from the Secretary of the Interior, and was prepared for the press from his nearly finished manuscript by Professor Jenney. Mr. Newton was a graduate of the School of Mines of Columbia College, New York; and the volume opens with a biographical sketch, by Professor Newberry of that School, of the young geologist, who died in 1877, while engaged in a second but private visit to the region for further explorations. The Report contains, after its historical introduction, a careful description of the successive formations of the region, which include besides the Archæan and volcanic or igneous rocks, the Silurian, Carboniferous, overlying "Red Beds" containing gypsum with some impure limestone referred provisionally to the Triassic, the Jurassic, Cretaceous. A large number of fossils were collected, and descriptions of them by Mr. Whitfield, with a general view of all thus far known from the Black Hills, occupy 135 pages of the volume, and their illustrations 16 of the plates.

The results show that the horizon of the Primordial beds is about the same with that of Wisconsin; that the Subcarboniferous and Permian groups could not be identified, while the Carboniferous is well represented by its mollusks and coals; that the Jurassic beds are full of fossils, as first made known by Hayden's survey in

1857, but have as yet afforded no Gasteropods; that all the formations are conformable to one another from the Cretaceous to the Primordial. The volcanic peaks occur over the part of the Hills north of the parallel of 44° 10′, without any linear arrangement or special relation in distribution. On the northeast margin of the Hills is Bear Butte; on the northwest side, in Red Valley, there are Inyan Kara, the two Sun Dance Hills, Warren Peaks, and another unnamed; on the Belle Fourche, Mato Tepee or Bear Lodge, the three Little Missouri Buttes; within the area, Custer, Terry (the crowning peak of the group), Crow Peaks and Black Butte, besides others less conspicuous. The rock of the cones is mostly sanidin trachytes, partly rhyolitic. No evidence of overflows was found, with a single small exception, as if there had been simply an extrusion of densely viscid material.

The peaks are cones, with sometimes regular craters, and vary in height above the valley at their base, from 300 to 1800 feet. Custer Peak is 675 feet above its base and 6,950 above the sea. Bear Butte is 1,200 feet above its base and only 4,570 above the sea, being about six miles from the edge of the foot-hills. Inyan Kara is 1,300 feet above the bed of the creek of the same name, and 6,600 feet above the sea.

Bear Lodge "is a great rectangular obelisk of coarsely porphyritic sanidin-trachyte, with a columnar structure, giving it a vertically striated appearance, rising 625 feet almost perpendicularly from its base. Its summit is so entirely inaccessible that the energetic explorer, to whom the ascent of an ordinarily difficult crag is but a pleasant pastime, as he stood at its base could only look upward in despair of ever planting his feet on the top." The height above the Belle Fourche is 1,126 feet, and its height above the sea approximately 5,260 feet; the width at bottom is 796 feet and at top 376 feet. In outline it is like the now unfinished Washington Monument. The columns of the columnar trachyte are over 600 feet in length and rise perpendicularly from a seemingly massive base. "It is exceedingly difficult," writes Mr. Newton, "to account for this structure as a result of cooling by comparison with any known basaltic phenomena."

Another remarkable feature of this locality is the undisturbed condition of the surrounding Potsdam sandstone; at a distance of but 50 to 75 feet from the base no evidence of any tilting could be detected, but the sandstone is "converted for some distance into a compact white quartzite."

The Little Missouri Buttes have a height of but 400 to 500 feet. They stand on the Dakota sandstone; but this floor-rock "could not be ascertained to exhibit any disturbance or change of structure due to the proximity of the igneous matter. The rock is greenish-gray trachyte, and there is also at the base, in one or two localities, (what is not mentioned as occurring about the other peaks) an exceedingly light and cellular rock, yellowish in color, very like a volcanic tufa or rhyolite breccia, including fragments of both sandstone and rhyolite.

From these facts the conclusion is arrived at that the time of eruption was later than the Dakota and Fort Benton groups of the Cretaceous and before the Miocene.

The rocks of the Hills were examined microscopically by Mr. J. H. Caswell, whose report occupies the last fifty-five pages of the volume and is illustrated by two colored plates. The report recognizes among the volcanic rocks trachyte, rhyolite, and phonolite, and the rhyolyte-trachyte was under the forms of volcanic glass, pitchstone, pearlstone, spherulite, etc. The trachyte includes sanidin-trachyte and sanidin-oligoclase-trachyte; biotite, hornblende, magnetite and apatite are often present, and the crystals of biotite have sometimes a border of magnetite. The phonolite contains much nephelite and some of it hornblende. The sanidin crystals in the trachyte from the top of Warren Peak are one to two inches long.

The volume contains also chapters on the Mineral Resources and climate of the Black Hills by WALTER P. JENNEY, on the botany, by ASA GRAY, and on the astronomical work of the expedition and the barometric hypsometry, by H. P. TUTTLE.

2. *Primitive Industry, or Illustrations of the Handiwork in Stone, Bone and Clay of the Native Races of the Northern Atlantic Seaboard of America*; by CHARLES C. ABBOTT, M.D. 560 pp., 8vo, Salem, Mass. (George A. Bates.)—Mr. Abbott has done good service to ancient American history in the preparation of this well systematized and well illustrated work. The region which he surveys embraces New England and the States of New York, New Jersey and Pennsylvania; but the wide range of his knowledge enables him to make comparisons with related facts from other parts of the country. Besides treating of implements of stone, bone and clay, the author mentions many examples of implements of copper and describes various shell-heaps—in all his chapters citing freely from previous publications on the subject.

The author's discoveries of flint implements in the stratified drift in the valley of the Delaware near Trenton, and the drift phenomena of the regions east, west and north, are the subjects of the two concluding chapters, the first of them by Dr. Abbott, and the second, on the antiquity and origin of the Trenton Gravel, by Prof. H. C. Lewis, of the Geological Survey of Pennsylvania. Dr. Abbott gives drawings of several of the specimens discovered, describes them as of hard argillite, and more rudely made than the ordinary implements of the country, and regards them as the work of the most ancient race of man on the continent, such as existed here before the disappearance of the ice of the Glacial era —and probably akin to the Eskimo. He refers to the occurrence of a tooth of the Reindeer (Rangifer Caribou), from the Trenton gravels, found by the late Prof. T. A. Conrad; to remains of the same species and of the bison, "in an ordinary rock-shelter" near Stroudsburg, Pennsylvania, along with marks of fire that suggested the idea of a feast on the bison by the men of the time; to the occurrence in New Jersey of antlers of the Greenland Rein-

deer "in the gravel that covers everywhere the older formations" mentioned by Prof. E. D. Cope (Geol. New Jersey, 1868, p. 740); and the long known facts respecting the existence in Kentucky of remains of the Moose, Caribou, Reindeer, Musk Ox and other northern Mammals; and regards Palæolithic man as a resident of the continent in the same era.

Mr. Lewis treats in detail of the stratified deposits of the Delaware, the position of the terminal moraine across the country, the origin of the deposits, and the antiquity of man. His conclusion is that the deposits are apparently post-Glacial, and probably were deposited by the flooded rivers at a period immediately following the last Glacial epoch upon the Delaware river. The view that there was more than one Glacial epoch over the region appears to require more evidence than has yet been presented; and if not a fact the opinion of Mr. Abbott will probably be sustained.

3. *M. E. Wadsworth on the Origin of the Iron Ores of the Marquette District.*—The notice of this paper, on page 320, does Mr. Wadsworth injustice. Its criticisms derived their force in part from the fact stated in the notice that, although the paper mentions the qualifications needed for successful investigation, it contains no "detailed facts, sections, or description of rock-slices," which the qualified investigator should have presented. Since the notice appeared Mr. Wadsworth has drawn my attention to the fact that his memoir "On the Geology of the Iron and Copper Districts of Lake Superior," published over a year ago, contains details of the kind asked for. I had overlooked this, having read and noticed the memoir, soon after its publication, as far as the subject of copper was concerned, but not its earlier half on the iron districts. This is a reason for the oversight but not an excuse for it. I take, therefore, this earliest opportunity to withdraw the derogatory remarks made in this connection. In addition I here transfer to this Journal, from his memoir, the larger part of the section on the "Jasper and Iron Ore," with a portion of his concluding statements. His account of his observations is illustrated by a number of figures showing the relations of the ore, jasper and schists, which are here omitted. He does not, however, give any drawings from microscopic views of thin rock-slices and, as he states, refers to the facts thus observed only in a general way.

I have also here to state that my remark on the banded structure (page 320), does not meet the argument he presents, which aims to show that since banding occurs in both igneous and metamorphic rocks, it cannot be distinctive of either.

With regard to Mr. Wadsworth's method of speaking of the labors and conclusions of others I have nothing further to say.

I add a word here on the bearing of the facts from other Archæan regions, especially those relating to the question of conformability, on the Marquette question, a point not appreciated in Mr. Wadsworth's discussions.

Of all the evidence used to prove a sedimentary origin of the

ore-deposits and schists, that of conformability between them is the one most relied on by geologists, and the most decisive. Mr. Wadsworth has considered it with reference to the Marquette iron ore, and, disagreeing with other observers, has decided the question adversely. But geologists who have studied, with that and other points in view, the widest range of Archæan iron regions—believing that they are alike in mode of origin—have reached the general conclusion that the ore and schists of all, the Marquette included, are conformable in bedding, and hence that they are metamorphosed sedimentary deposits. My own examinations on this point, at localities in New Jersey, New York and Connecticut, have confirmed me in the same view; and, with others, I believe it will be found that any apparent unconformability in bedding is local and a consequence of the disturbances —the flexures, fractures, faultings and attendant changes—which these oldest of beds have undergone. J. D. D.

4. *On the Jasper and Iron Ore of the Marquette Region;* by M. E. WADSWORTH.—In the Marquette region, the country rock is of a varying nature, but is mainly composed of schists (largely chloritic), argillites, and quartzite, in that part of the district visited by us. Associated with these rocks is the jasper, which is acknowledged on every hand to be an inseparable part of the iron ore formation. The origin of one gives the origin of the other. Their interdependence is such, and has been so regarded, that the relations of one to the country rock give the relations of the other. The two have been so fully described in the past, that it is only necessary to briefly describe them here.

The common form is that of interlaminations of jasper and iron ore, the laminæ varying from extreme tenuity to considerable thickness. In some places the jasper predominates, in others the ore. In the last case we have a more or less valuable ore, according to the amount of the siliceous material, which, however, may exist only in a mere trace. The purer parts form large masses, that are mined, and which graduate into the jasper, or ore containing so much jasper as to be unfit for working. The workable parts are frequently lenticular in form, although often irregular. The irregularity of the ore mass, its passage into the jaspery ores, and the uncertainty where the next mass will be found, are among the chief difficulties of the miner. The origin of the jasper and ore becomes then a problem of great economic importance, as do also the relations of both to the country rocks. The permanence and extent of the formation, whether it is in the form of vein deposits, eruptive (intrusive or overflow) masses, or sedimentary deposits, are questions in which the capitalist and miner, whether they will or not, are most deeply interested. As they have never been regarded as vein deposits, there remains for us only the question whether the jasper and its associated ores are eruptive or sedimentary in origin.

Lest there be some misunderstanding as to the reason for thus dismissing the theory of the ores here being vein deposits, we

would remark that the question has been ably and fully discussed before in the works of previous observers. Furthermore, while veins on a small scale are occasionally seen, we were unable to find upon either the jasper or its associated ore a single character belonging either to a vein or an infiltration deposit. It therefore seems unnecessary to discuss the vein or infiltration theory here.

As both the eruptive and sedimentary origin of the jasper and the ore have been advocated by some of the most eminent geologists in this country, it is necessary that the question should be answered by the facts, and not by any preconceived theory or idea. The question now is what are the facts, and their most probable explanation. The first and most important thing to be observed in deciding this is the relation of the jaspery formation to its country rocks.

This relation is well shown in and about the Lake Superior mine at Ishpeming. On the north side of one of the abandoned pits just east of the main workings, the junction of the jasper and ore with the chlorite schist was observed and figured. Specimens were also taken that show the contact. The junction of the two is very irregular, the banding of the jasper and ore following the irregularities of this line, while the schist is indurated and its laminæ bear no relation to the line of contact. Stringers of ore project into the schist, which near the jasper is filled with octahedrons of magnetite. The schist loses its green color generally, and becomes apparently an indurated argillite. The contact and relations of the two rocks are not such as are seen when one sedimentary rock is laid down upon another, but rather that observed when one rock is intrusive through another; and in this case the intrusive one is the jasper and its associated ore. On the south side of the same pit the jasper bows in and out in the schist, forming at one place a projecting knob whose banding follows its contour. Lying against it is a long arm of jasper, similarly banded, which ends in a rounded knob. In the southwest corner of the same pit a dike of very fair hematite ore about one foot in width breaks at an angle of 15° across the argillite and schist, whose lamination is vertical. Wherever the unbroken contact of the jasper and ore with the schist could be observed, that junction is seen to be an eruptive one, on the part of the former. At the School-house mine east of the Lake Superior mine, the jasper forms a dike with a knob-like ending, the lamination (banding) following the curvature of the sides. The contacts between the ore and schist were well-marked eruptive ones. Overlying the ore was found on one side a ferruginous and quartzose breccia and conglomerate composed principally of the ruins of the underlying ore and jasper. A similar but finer-grained rock, mostly a quartzite, forms the hanging, or better the fallen wall of the New York mine. This is composed, in like manner, chiefly of the *débris* of the underlying ore and jasper. Mr. Brooks's statement regarding the "quartzite" of the Marquette district is undoubtedly true of this rock, that when he finds the "quartzite," adja-

cent to it will be found all that is left of the ore formation. This, however, is not what Mr. Brooks intended in his statement, as these detrital rocks apparently form but a small portion of his "quartzites." These of course mark old beaches water-worn after the jasper and ore were *in situ*, in nearly their present condition and, if the logic of the geologists of the Michigan and Wisconsin surveys were carried out, these unconformable detrital formations would mark a new geological age. * * *

At the upper portion of the Jackson mine, Negaunee, the jasper and hematite were seen to cut across and obliquely up through the schists. The jasper also curves in a similar manner at right angles to this nearly east and west section. While this could be explained easily by sedimentation, it is fatal to the view of conformable deposition. In pit No. 3 of this mine (Jackson) the ore breaks irregularly through the schist, forming a brecciated-looking mass, while in other cases it runs up into the schist ending in irregular knobs. * * * *

In pit No. 4 a wedge of ore and jasper was seen intruding between and across the lamination of the schist. In the "north pit" the eruptive character of the ore is well shown. Overlying the ore at a low angle is a quartzite containing jasper and ore derived from its underlying ore. At the Home mine in the Cascade range the ore was largely a sandstone impregnated with hematite, strike N. 70° W. with a northerly dip, which varies owing to the contortion of the strata from 30° to 70°. Several dikes of jasper run through this sandstone, in part conforming to the bending of the strata, and in part breaking across the laminæ. There is no mistaking the intrusive character of the jasper and its interlaminated ore here. It is of course almost unnecessary to state that this mine, having as its chief ore a ferruginous sandstone, was long since abandoned. The quartzite (metamorphosed sandstone), which forms the hanging wall of the Pittsburg and Lake Superior mine, Cascade range, has been cut through by dikes and little stringers of nearly pure hematite which, in its present position, is distinctly intrusive. While in general these little dikes follow approximately the bedding, they are seen not to exactly do this, but cut the laminæ obliquely through much of their course. This mine contains as a secondary formation much specular iron. Near the bridge over the Palmer mine the jasper shows well its eruptive character in its junction with the quartzite, while the banding is seen to be parallel to the contact line. This jasper holds in it, and as part of itself, the hematite mined here.

It is advocated by Messrs. Credner and Brooks that all the iron was originally in the state of magnetic oxide, this view being sustained by the crystals of martite found in various parts of the district.

It would seem that a microscopic examination of the banded jasper and ore should give us some facts bearing upon the question. A section was made of a finely-banded jasper taken near

the Lake Superior mine. Under a lens this shows a fine contorted banding. Microscopically this section is composed of a fine granular aggregate of quartz and hematite, and a more coarsely crystallized portion made up of octahedrons of magnetite or martite, and of quartz of secondary origin. The quartz in the first part is largely filled with minute globules and grains of ore, which also occurs in irregular masses and in octahedrons. The quartz associated with the more coarsely crystallized portion is water clear, and shows the usual fibrous granular polarization of secondary quartz. Wherever the iron is in a distinguishable crystalline form it is in octahedrons. The color and streak of the iron in the hand specimen are those of hematite, but the powder is found to be magnetic. The section was taken from the most jaspery portion, and shows much of the fine aggregation of quartz and hematite. The structure of the quartzose portion is like the devitrification structure of the rhyolites and felsites. The section has been repeatedly fissured, and the fissures filled in with secondary deposits of quartz and octahedral crystals of iron. So far as we have observed, the brecciated jasper and ore have had their fractures filled in like manner. The jaspery portion is finely banded, and shows an apparent fluidal structure. We are inclined to regard the structure as fluidal, but in a rock so deeply colored it is difficult to make satisfactory examinations. This is the only section that shows anything like a well-defined limit between the jasper and ore bands, under the microscope, as pointed out by Dr. Wichmann.* The powder of the two last-described specimens is feebly magnetic. The quartz is much fissured, showing the effect of heat, and contains microlites and fluid and stone inclusions.

The octahedral form of the iron ore would sustain the view that it was all originally magnetite. The difficulty lies in proving the crystals to be primary, and not secondary forms, especially as they are largely associated with secondary quartz, and also are abundant in the little fissures (minute veins), traversing the jasper. Our microscopic examination of rocks of various ages and characters goes to show that all rocks, especially the older, have been subject to more or less alteration. This alteration is accompanied by recrystallization, which often obliterates the original characters. This change appears to be produced through the medium of the percolating waters, and consists rather in a chemical rearrangement of the constituents of the rock, amongst themselves, than in the deposition of any material brought in from extraneous sources. The jasper and iron ores, as well as all other rocks examined microscopically from this district, have suffered this alteration to a greater or less extent; therefore it is perhaps impossible at present to be sure of the original state of the iron, or how many changes have taken place.

Without objecting in any degree to the idea that the ore was originally magnetic, certain facts indicate that the present mag-

* Geol. of Wisc., iii, 615.

netic state of the iron is in some places due to secondary causes; i. e. the heat of intrusive rocks erupted since the iron ore and jasper were in place. While in general the Republic Mountain ore is hematite, exceptions exist. On the northerly side of the hill a "diorite" dike was seen. It is found that the ore was so affected by the heat of this intrusive mass that it is magnetic adjacent to it, while a short distance away it is the normal hematite. Numerous other localities were examined about the hill where these secondary intrusions occurred, with the same result; the iron ore was magnetic adjacent to the dikes, but not magnetic a short distance away. As a general rule, the magnetite or the hematite pseudomorphs after it (martite) are found near the "quartzite" of Brooks in this mine. Those who examine the map of Republic Mountain, prepared by him,* will observe on the northern side ot his "quarztite," a queer tongue of it projecting into the hematite. An examination of this tongue at different places shows the following facts: It contains numerous rounded and irregular fragments of the iron ore in it; these fragments occur on both edges, while the centre of the mass is free from them. At this point it varies from a few inches to two feet in width, and it is seen to break across the lamination, although nearly coinciding with it. At another part shown near the same pit, this rock and its contact with the "jasper" and ore were well marked. The "quartzite" is firmly welded to the ore, and breaks across the laminæ, cutting them, and sending tongues into the mixed jasper and ore. The junction is an eruptive (intrusive) one, and not that belonging to the contact of one sedimentary rock with another. The ore at the junction is magnetic. The question whether this is an intrusive or sedimentary rock has another side than the simple scientific one. It makes a great difference in the mine whether this is a simple overlying metamorphosed sandstone, as Mr. Brooks places it, or a later intrusion cutting the ore below. This latter case opens up numerous questions that the practical man can only disregard to his cost, sooner or later, but which have nothing to do with the present discussion.

As this rock seems to belong to the granites, it will be described under them. Should future research show that all of the "quartzite" of Republic is the same as the tongue is, it would have a bearing on the proximity of the magnetite and martite to it.

In like manner, passing to other mines where secondary intrusions are more abundant, the magnetite becomes a more prominent feature. It seems, so far as we have seen, that the magnetite and martite are directly proportioned to the amount and proximity of eruptive rocks, extravasated since the ore was *in situ.*—*Geol. Iron and Copper Districts*, pp. 28–35.

From the "General discussion," in the last chapter, on the Iron District.—So far as geological science has now advanced,

* Atlas, Geol. of Mich., 1869–73, Plate VI.

the facts observed can only be explained by the eruptive origin of both the ore and jasper, as they make the same formation.

The ore and jasper show that they are the intrusive bodies by their breaking across the lamination of the schists and other rocks, by the changes that take place in the latter at the line of junction, by horses of schist being enclosed in the ore, by the curvature of the lamination, produced by the intrusion of the ore and jasper, etc. Not the slightest sign of the plasticity or intrusion of the schists relative to the ore or jasper was seen. That the present lamination of the schist existed prior to the intrusion of the ore and jasper is shown by the effect of the latter upon and its relations to it. That this lamination is the original plane of deposition is for part of the schists not known; but whether it is or not, it has been taken to be such by the observers quoted in the establishment of their theories, and they must abide by it. The lamination, however, coincides with many of the well-stratified rocks adjacent, and in some of these the ore and jasper were unmistakably intrusive. The schists that retained well-marked stratification planes showed in some places extraordinary contortions, one specimen showing a synclinal and anticlinal fold, requiring, were the top eroded, the counting of the same layer four times in the width of two inches. This is only one case out of numerous ones observed.—*Ibid*, p. 67.

5. *Saurian and Mammals of the Lowest Eocene of New Mexico.*—Professor Cope has described in the American Naturalist (August, 1881), a Saurian, *Champsosaurus australis* Cope, from beds in New Mexico which lie below the typical Wasatch Eocene, and possibly from the Puerco beds. The genus was first described by Cope from specimens in the Laramie beds named *C. laticollis*, and has since been recognized by Dr. Lemoine, near Reims, in the Suessonian Eocene associated with mammals. From these same lowest Eocene beds of New Mexico Prof. Cope has described the mammals, *Mesonyx Navajovius* (Creodont), *Periptychus carinidens* (Creodont), *Triisodon Quivirensis* (Creodont, which group is placed by the author between the Marsupials and Carnivores), *Deltatherium fundaminis* (Creodont), *Conoryctes comma*, allied to Mesonyx, *Catathlæus rhabdodon*, *Anisonchus sectorius*, *Mioclænus turgidus*, *M. subtrigonus*, *Phenacodus Puercensis*, *Ph. Zuniensis*, *Protogonia subquadrata* (Chalicotheriidæ), *Meniscotherium Terrærubræ.* No Coryphodon remains have yet been detected in the beds. The Suilloid genera are stated to be characteristic.

6. *Miocene Rodents of North America and Canidæ of the Loup Fork Epoch.*—A review of the N. A. Miocene Rodents and another of the Canidæ of the Loup Fork Epoch is published by Professor Cope in vol. vi of the Bulletin of the U. S. Geol. Survey under Dr. Hayden, for September, 1881.

7. *The Irish Elk, Megaceros Hibernicus, in the Ancient lake deposits of Ireland.*—Mr. W. Williams, in the Geological Magazine for August, describes the deposits of some of the bogs of Ire-

land, with reference to the position in them of bones of the Irish Elk. Those of the Ballybetagh bay, about nine miles southeast of Dublin, have first (1) a lining of tenacious clay resting on bowlder clay, within this at bottom (2) a yellowish gray bed, slightly clayey, consisting chiefly of vegetable matter; next (3), a bed of dark brownish clay containing remains of Megaceros; then (4), a grayish clay about thirty inches thick, containing rock debris from the hills; which last is covered by peat. He states that the bones are found only in No. 3, and that during the thirty years past, nearly one hundred heads have been found in this bog (almost all males), with scarcely six skeletons. The stage of growth of the antlers—whose average weight is sixty pounds—shows that the animals were mired at different times during the year.

The clay No. 4 is regarded by the author as having been deposited during the second Glacial epoch, and the stones it contains are attributed to the ice and frosts of that time. In this clay the author found the antler of a Reindeer, and this is regarded as corroborative of his conclusion. The broken state of the bones of the Megaceros is attributed to the pressure of the overlying mass or masses of ice. No human implements occur in the clay, leading to the conclusion that "man had hardly appeared in Ireland," and that the Megaceros was exterminated not by man, but by the augmenting cold of the approaching Glacial era. All these inferences are stated to be sustained by the facts from other Irish bogs.

8. *The Tertiary Lake Basin of Florissant, Colorado;* by S. H. SCUDDER. pp. 279–300 of the Bulletin, vol. vi, No. 2, of the U. S. Geol. and Geogr. Survey, under Dr. F. V. Hayden (Dept. of the Interior).—Mr. Scudder describes in this paper the position, characters, paleontology and age of the remarkable lacustrine deposits of Florissant, Colorado, and illustrates the subject with a map. His observations in the region were made in 1877, along with Mr. A. Lakes, whose geological notes are incorporated, and also Mr. F. C. Bowditch. The lake-basin, nearly nine miles long, according to the map, occupies a low depression among the mountains at the southern extremity of the Front Range of Colorado "at no great distance from Pike's Peak," and sends its arms up the valleys on either side. The beds are whitish, drab and brownish shales below, with fine and coarse sandstone above; and, besides, trachyte occurs in the adjoining promontories and along the margin of the basin. The material of the coarser beds directly above the shales, from a locality visited by Mr. Scudder (south of the house of Mr. A. Hill), according to microscopic investigations by Mr. M. E. Wadsworth, is tufaceous; and the shales are "simply the finer material of the tufas laid down in laminæ of varying thickness and coarseness." The shales at this place are about 22¼ feet thick. The fossils from the Florissant shales include:—of Hymenopterous insects, several species of Apidæ and Andrenidæ, about 30 of Vespidæ or wasp-like Hymenoptera, 50 species or more of ants (mostly Formicidæ, with

some Myrmicidæ and Poneridæ) represented by about 4,000 specimens; about 80 species of Ichneumonidæ, over 100 other species of Hymenopters; of Lepidopters perhaps a dozen species; of Dipters, some thousands of specimens and a large number of species, among them 1,000 specimens of Bibionidæ, and "a vast host of Muscidæ and allied kinds;" of Coleopters, over 300 species, of the normal series, and about 120 of the Rhyncophorous section; of Hemipters, more than 100 species of the Heteroptera, and 65 of Homoptera; of Orthopters, many species; of Neuropters, largely the Phryganidæ of which there are 15 or 20 species, 6 species of the Termites family, and others; of Spiders, 30 species, all Araneæ; one Myriapod, an Iulus; of Mollusks, only one species, that a Planorbis; of Fishes, 8 species, all described by Cope, except one by Osborn, Scott and Speir; of Birds, several feathers, a single tolerably perfect Passerine bird, described by J. A. Allen, under the name *Palæospiza bella*, and a plover, *Charedinus Sheppardianus*, described by Cope.

The fossil plants include large silicified trunks of trees probably Sequoias, and many species, 90 to 100 in all, about 40 of which have already been described by Lesquereux, besides some flowers with long stamens. The assemblage of plants indicates, according to Lesquereux, a climate like that of the northern shores of the Gulf of Mexico; of fishes, according to Cope, of latitude 35°; of insects, according to Scudder, a still warmer climate.

The age of the deposits is referred by the most recent and best authorities to the later Eocene or early Miocene.

The insects are soon to be desbribed by Mr. Scudder in a quarto volume and illustrated by a large number of plates.

9. *Address of the President of the Geological Society of London*, ROBERT ETHERIDGE, F.R.S., at the Anniversary Meeting on the 18th of February, 1881.—The subject of this address, is the "Analysis and Distribution of the British Paleozoic Fossils." It is a carefully prepared and critical review of what has been learned respecting the ancient life of Great Britain in Paleozoic time, drawn up with details as to the species of plants and animals in the successive formations, and as to their stratigraphical and geographical distribution, and it has a special interest for the American geologist, on account of the wide extent and thickness and abundant fauna of related rocks on this side of the Atlantic.

10. *Pantotheria of Marsh.*—This word is incorrectly spelled *Prototheria* on page 286 of this volume.

11. *Occurrence of Vanadates of Lead at the Castle Dome Mines in Arizona;* by WM. P. BLAKE.—The occurrence of various vanadium minerals at different points in Arizona has been recently described by Professor Silliman in this Journal. Similar observations were communicated to the Mining and Scientific Press of August 13, by Professor Wm. P. Blake. He states that vanadinite occurs in considerable abundance at the "Railroad" claim in the Castle Dome district. It forms groups of small hexagonal prismatic crystals, generally curved and tapering as is common in pyromorphite. It is also found in

crusts of confusedly aggregated crystals, sometimes filling cavities in decomposing ores of lead and sometimes fluor spar. The color of the larger crystals is generally brown; the smaller ones are lighter and are of various shades of orange, yellow, and yellowish-brown, the latter of a wax-like luster. Associated with the vanadinite are possibly some rarer vanadates, not yet identified, wulfenite, in brilliant light-yellow crystals, and a vanadiferous mimetite which seems to graduate into the pure vanadinite. Professor Blake also mentions the occurrence of beautiful crimson-red crystals of vanadinite from the Hamburg mine and fine wulfenites, sometimes in octahedral crystals, from the Red Cloud mine, the "Oakland Boys' claim," and other points in the Silver District, Arizona.

III. Botany and Zoology.

1. *Recent papers on the Marine Invertebrata of the Atlantic coast of North America;* by A. E. Verrill.—During the past few years a much more active interest has been taken in the marine invertebrata of our coast than ever before, and accordingly there has been a rapid increase in the number of papers published on this subject. This has been due principally to the extensive explorations of the sea-bottom and its life, made by the U. S. Coast Survey and the U. S. Fish Commission. The work done by the Coast Survey was mostly in the southern waters, in the Gulf of Mexico, Carribbean Sea, and off Florida, but in 1880, included lines of dredging off the eastern coast of the United States, and even to the region off George's Banks. This work, so well begun by Pourtales, has, during the later years, been carried on with great perseverance, and with remarkable success by Mr. A. Agassiz, whose collections, made by the steamer "Blake" are of wonderful extent and interest. Numerous reports on the earlier of these collections have been published, during past years, but in the following list, I include only the more recent ones.

The explorations by the U. S. Fish Commission, under the supervision of the writer, have been mostly along the northern coast, from Long Island Sound to Nova Scotia, and in water of moderate depths, usually within 100 miles of the coast. But all this region has been very fully examined, dredgings having been made at over 1600 stations, while collections of very great extent have been accumulated. As yet very few of the final reports on these collections have been published, but numerous preliminary papers, by the writer and others, have been printed. Among the more recent papers relating to the Fish Commission collections, in addition to those printed in this Journal, are the following:

Report on the Marine Isopoda of New England and Adjacent Waters. By Oscar Harger. <*Report of the United States Commission of Fish and Fisheries*, Part VI, for 1878 [pp. 297–458, 13 plates], 1880.—This is a complete monographic report on all the species (46) known up to the date of publication, with descriptions and good figures of nearly all the species. It is followed by a bibliographical list of works on the subject.

Report on the Pycnogonida of New England and Adjacent Waters. By Edmund B. Wilson. <*Report of the United States Commission of Fish and Fisheries*, Part VI, for 1878 [pp. 463-504, pl. 1-7], 1880.—A monographic revision of all the species—fifteen in number—followed by a bibliographical list.

Notice of a new Species [*Polycheles sculptus*] *of the "Willemoesia Group of Crustacea," (Recent Eryontidæ).*<*Proceedings U. S. National Museum*, vol. ii, for 1879, [pp. 345–353, pl. 7], March, 1880. By Sidney I. Smith.

Preliminary notice of the Crustacea dredged in 64 *to* 325 *fathoms, off the south Coast of New England, by the United States Fish Commission in* 1880. By S. I. Smith.<*Proceedings U. S. National Museum*, vol. iii, for 1880, [pp. 413–452], January, 1881.—Contains a general list of fifty species, with their geographical distribution and descriptions of numerous new species and one new genus (Hemipagurus).

Notice of Recent Additions to the marine Invertebrata of the northeastern coast of America with descriptions of new genera and species and critical remarks on others. <*Proceedings of the United States National Museum*, vol. iii, Dec., 1880, and Jan., 1881.

Part II.—Mollusca, with Notes on Annelida, Echinodermata, etc., collected by the U. S. Fish Commission [pp. 356–405], Dec., 1880 and Jan., 1881.

Part III.—Catalogue of Mollusca recently added to the Fauna of Southern New England [pp. 405–409]. By A. E. Verrill.

The Cephalopods of the Northeastern Coast of America. By A. E. Verrill. *Part II.—The smaller Cephalopods, including the "Squids" and Octopi, with other allied forms.* <*Trans. Conn. Acad.*, v, [pp. 259–424 (unfinished), pl. 26–56], June, 1880, to Oct., 1881.—Although this paper is all in type, a few of the last signatures are not yet issued. It is a monographic revision, with descriptions and figures of all the species. A considerable amount of anatomical work is also introduced. Most of the species have already been noticed by me, in different articles, in this Journal. Among those not previously described are *Chiroteuthis lacertosa*, *Brachioteuthis Beanii*, gen. et sp. nov., *Rossia megaptera*. *Brachioteuthis* is a deep-sea genus, allied to *Chiroteuthis*, but having simple connective cartilages on the siphon and mantle. A new genus (*Stoloteuthis*) has also been established for *Sepiola leucoptera* V. It is remarkable for having *free* eye-lids, round pupils, arms webbed, and no pen. *Inioteuthis* is established for *Sepiola Japonica*; it differs from *Sepiola* in lacking a pen. A second Japanese species has four rows of suckers (*I. Morsei*).

New England Annelida. Part I, Historical Sketch, with Annoted Lists of the Species hitherto Recorded. By A. E. Verrill. <*Trans. Conn. Acad.*, iv [pp. 285–324], 1881.—In connection with the annotation, a considerable number of changes in nomenclature are introduced, and a few new genera are established. These are *Euglycera* for Glycera dibranchiata Ehlers; *Dipoly-*

dora, for Polydora concharum V.; *Praxillella* for Praxilla (preoccupied). The several species hitherto referred to Anthostoma are referred to *Scoloplos*.

The following papers relate to the collections made by Mr. Agassiz, on the "Blake:"

Reports on the Results of Dredging, under the supervision of Alexander Agassiz, by the United States Coast Survey Steamer "Blake." Bulletin of the Museum of Comparative Zoölogy, Vols. viii, ix.

VIII.—Etudes préliminaires sur les Crustacés. Par A. MILNE-EDWARDS. *Ier Partie*, viii, [pp. 1–68, 2 pl.], Dec. 29, 1880.—Contains brief descriptions of a large number of new genera and species of Decapoda. Many of them can scarcely be identified without figures.

IX.—Preliminary Report on the Echini. By A. AGASSIZ. Vol. viii, [pp. 69–84], Dec., 1880.—Enumerates 45 species, of which many are described as new.

X.—Report on the Cephalopods, and on some additional species dredged by the United States Fish Commission Steamer "Fish Hawk," during the season of 1880. By A. E. VERRILL. Vol. viii, [pp. 99–116, 8 plates], March, 1881.—This includes descriptions of two new species, viz: *Mastigoteuthis Agassizii*, gen. et sp. nov., *Eledone verrucosa*. Figures and descriptions are also given of Chiroteuthis Bonplandi? [= *C. lacertosa* V., 1881], Rossia sublevis, Heteroteuthis tenera, Octopus Bairdii, O. lentus, Cheloteuthis rapax V. [=Lestoteuthis Fabricii V. [=Gonatus Fabricii Steenst.], Calliteuthis reversa, Alloposus mollis.

XI.—Report on the Acalephæ. By J. W. FEWKES. Vol. viii, [pp. 127–140, 4 plates], March, 1881.—Contains descriptions of several new species of *Plumularidæ*, *Sertularella*, *Lafoëa*, and of a new genus, *Aglaophenopsis*.

XIII.—Report on the Pycnogonida. By E. B. WILSON. Vol. viii, [pp. 239–256, 5 plates], March, 1881.—Ten species are recorded. One genus and five species are new. The new species belong to *Colossendeis*, *Scæorhynchus* (nov.), and *Pallenopsis* (nov.) Some of the species of *Colossendeis* are of great size (extent, 343^{mm}).

XV.—Preliminary Report on the Mollusca. By W. H. DALL. Vol. ix, [signatures 3–6, pp. 33–96, unfinished], July to September, 1881.—A large number of new species and several new genera are described in the four signatures received. Among the genera treated are Cadulus, Dentalium, Siliquaria, Pedicularia, Margarita, Calliostoma, Seguenzia, Basilissa, Septothyra, Callogaza, nov., Microgaza, nov., Fluxina, nov., Hanleyia, Pleurotoma and its subdivisions, Trichotropis, Marginella, Puncturella, Pleurotomaria, Haliotis, Crepidula, Triforis, Cerithiopsis, Bittium, Columbella, Natica, Turritella, Actæon.

XVI.—Preliminary Report on the Comatulæ. By P. HERBERT CARPENTER. Vol. ix, [20 pp. 1 pl.], Oct. 1, 1881.—The author states that he now recognizes about 55 species of this group from the Caribbean Sea and West Indies. They belong

mostly to the genera *Actinometra* and *Antedon*. Of these the former is the more abundant, both in species and individuals. In this paper a few new species of these genera are described, and two species of a new genus, *Atelecrinus*. The "Blake" collection is contrasted with that of the "Challenger."

Various other papers, not relating particularly to the two explorations referred to above, have been recently published. Among these are the following:

The Stomach and Genital Organs of Astrophytidæ. By T. LYMAN.<*Bull. Mus. Comp. Zoology*, viii, [pp. 117–126, 2 plates,] Feb., 1881.

Studies of the Jelly-Fishes of Narragansett Bay. By J. W. FEWKES.<*Bull. Mus. Comp. Zoology*, viii, [pp. 141–182, 10 pl.]. —This includes many details concerning several known species and the following new forms: Mabella gracilis, gen. and sp. nov., Modeeria multitentacula, Dinematella cavosa, gen. and sp. nov., Eutima gracilis, Sphærula formosa, gen. and sp. nov., Cunina discoides.

II.—The Siphonophores. The Anatomy and Development of Agalma (continued). By J. WALTER FEWKES. <*American Naturalist* [pp. 186–195], March, 1881.

On some Points in the Structure of the Embryonic Zoëa. By WALTER FAXON.<*Bull. Mus. Comp. Zoology*, vi, [pp. 159–166, 2 plates], Oct., 1880.—Relates to the development of *Carcinus* and *Panopeus*.

On some Crustacean Deformities. By W. FAXON.<*Bull. Mus. Comp. Zoology*, viii, [pp. 257–274, 2 plates.]—Discusses numerous deformities of *Homarus* and *Callinectes*.

The Development of the Squid, Loligo Pealii (Lesueuer). By W. K. BROOKS.<Anniversary Memoirs of the Boston Society of Natural History, 1880.

Annelida Chætopoda of New Jersey. By H. E. WEBSTER. <*Thirty-second Annual Report on the New York State Museum of Natural History*, [pp. 1–28, *plates not issued*] 1880, (*dated* 1879).—Although put in type in 1879, this paper was not actually published until 1880, and the plates that were prepared for it have not yet been published. A number of new species are described belonging to the genera Anaitis, Eteone, Podarke, Grubea, Goniada, Polydora, Streblospio (gen. nov.), Praxilla, Paraxiothea (gen. nov.), Sabellides. Fifty-nine species are enumerated.

2. *A Manual of Practical Normal Histology;* by T. MITCHELL PRUDDEN, M.D. (New York: G. P. Putnam's Sons, 1881.) pp. viii and 265, small 8vo.—The method of giving a brief description of the tissues and organs in appropriate sequence, and following each description with an account of the way in which the structures described may be demonstrated has been admirably carried out in this modest little volume, which well fills an unoccupied place among elementary text books. In no other English text book certainly can be found so concise and clear an account of the structure of the principal animal tissues, as at present understood. The directions for demonstration are sufficiently

simple and clear to be readily followed, even without an instructor, by any intelligent student familiar with the use of the microscope. S. I. S.

3. *U. S. Entomological Commission, Department of the Interior.* Index volume to Dr. Riley's Missouri Reports on Insects.—Bulletin No. 6 of this Commission, numbering 178 pages, 8vo, consists of a General Index and Supplement to the Nine Reports by Charles V. Riley, M.A., Ph.D., on the Insects of Missouri. The importance of these reports, economically and scientifically, makes this Index volume one of much value. It is intended to stand as vol. x of the series. To increase the value of the volume, the author has brought together the tables of contents of the nine volumes, with errata, and has also reproduced the descriptions of new species, added a list of descriptions of adolescent states, of descriptions of species not new, of food plants, and of illustrations.

4. *The Hessian Fly, its ravages, habits, enemies, and the means of preventing its increase*, by Dr. A. S. PACKARD, is the subject of Bulletin No. 4. It is illustrated by a map and two plates.

5. *E. S. Morse on changes in Mya and Lunatia.*—Professor MORSE has sent the following correction for his note on page 323 of this volume: "A comparison of the common beach cockle Lunatia showed that the present form living on the shore to-day had a more depressed spire than the ancient form."

IV. ASTRONOMY.

1. *Theory of the Moon's Motion, deduced from the Law of Universal Gravitation*; by JOHN N. STOCKWELL, Ph.D. Philadelphia, 1881. (J. B. Lippincott & Co.)—Although the motion of the moon around the earth has been the subject of profound study, during the past two hundred years, and has been the occasion of more elaborate mathematical investigations than all the other members of the solar system together, the tables of the moon's motion which are based on these calculations fail to represent the moon's place in the heavens with a precision at all commensurate with the labor bestowed upon the lunar theory. As a matter of fact the latest tables of the moon scarcely represent the observations with greater precision than those in use at the beginning of the present century. In view of this fact, the author of this work several years since called the attention of astronomers to the great apparent errors of the lunar theory, and expressed the belief that the theory itself must be in error by terms of the third order of magnitude in the perturbations, instead of being correct to terms of the seventh order, as had been hitherto supposed.

In order to satisfy himself of the correctness of this conclusion, he undertook, some six years ago, a complete and systematic development of the lunar theory, according to a method different from any that had been before applied to the problem. The

application of this method to the motion of the moon constitutes the principal, or strictly technical part of the work; while the history of the problem and the comparison of the results obtained, with the corresponding results of other calculators, is given in considerable detail in the introduction.

The author believes that he has discovered two equations of the third order of magnitude having a short period, besides other terms of long period depending on the sun's action and on the oblateness of the earth, which have not been before correctly computed, and which are of very considerable importance in the lunar theory. Should these results be confirmed by other calculators, the remark of the late Astronomer Royal, Sir G. B. Airy, made some eight years ago, namely: "that there is some serious defect in the lunar theory," will be fully justified.

This is the first book published in this country, which is wholly devoted to the mathematical development of the *general theory* of the moon's motion, as affected by the sun's attraction; although important papers have been published at different times in the various scientific journals of the country, upon some particular cases of lunar perturbation due to the sun's action. The problem, though old, is still one of the most interesting and important in celestial mechanics; and it is to be hoped that other American mathematicians will interest themselves in its solution.

The book contains about *four hundred* pages, and in this respect contrasts happily with the great works of Plana and Delaunay, which, taken together, cover considerably more than *four thousand* pages.

It is printed in handsome style, on excellent paper. Aside from the intrinsic importance of the subject of which it treats, it furnishes a multitude of beautiful solutions of problems which are always of interest to the student of the pure mathematics; and we cordially commend it as worthy of a place in all the scientific libraries of the country, whether public or private.

2. *Astronomical and Meteorological Observations made during the year* 1876 *at the U. S. N. Observatory.* Rear-Admiral C. H. Davis, Superintendent. Government Printing Office, 1880.—This contribution of the Naval Observatory to science fills two thick quarto volumes, one containing the regular observations and the other made up of three important appendices. The first appendix is a subject index (of 74 pages) prepared by Prof. Holden, to all the publications of the U. S. N. Observatory. It makes the valuable material that is scattered through these volumes far more easily accessible than heretofore to astronomers. The second appendix (of 126 pages) contains the several reports on the transit of Mercury, in May, 1878. The third appendix, on the solar eclipse of July, 1878, has been already noticed (this Journal, III, vol. xxi, p. 334). H. A. N.

OBITUARY.

Dr. G. Linnarsson, paleontologist of the Geological Survey of Sweden, died recently at the age of forty years.

THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

Art. LIV.—*On a possible cause of the Variations observed in the amount of Oxygen in the Air*; by Edward W. Morley, M.D., Ph.D., Hurlbut Professor of Chemistry, Western Reserve College, Hudson, Ohio.

In order to determine, if possible, whether the observed variations in the proportion of oxygen contained in the atmosphere at different times is occasioned by the descent of air from an elevation above the surface of the earth, I have made two series of analyses in duplicate of samples taken each day at this place. The results are given in the accompanying table.

The results for the first period have been compared graphically with the indications of the thermometer and barometer furnished me by the Signal Service observer at Cleveland. At times, when the meteorological conditions of the region were simple enough, it was easy to see that the deficiencies in the proportion of oxygen followed closely times of high barometer and low temperature, when, if ever, it would be fair to infer that the descent of air from an elevation would take place. But in a great number of cases it is impossible to infer very much as to the atmospheric currents of the region from observations at one place.

But if we examine the thrice-daily maps of the state of the thermometer, barometer and winds, as observed by the Signal Service, we may obtain reasonably good evidence as to the atmospheric currents of this region at the times of deficiency of oxygen observed. When we find this place at or near a center of high pressure, and find the recorded directions of the

Oxygen found in Air at Hudson, Ohio, from January to December, 1880.

January, 1880.

Date	Oxygen	
1	956	969
1^{1}	962	955
2	959	957
3	976	
4	959	954
5^{1}	947	948
6^{3}	958	968
7^{3}	955	958
8^{4}	941	936
9^{2}	936	
10^{4}	947	944
11^{4}	960	965
12^{3}	963	
13^{4}	955	965
14^{4}	967	952
15^{4}	960	
16^{2}	954	
17^{3}	969	
18^{4}	939	
19^{2}	962	
20^{4}	954	947
21^{3}	928	933
22^{4}	948	949
23	943	946
24	947	954
25	952	956
26	931	
27	941	947
28	951	
29^{5}	922	931
30	939	
31	965	959

February.

Date.	Oxygen.	
1	949	945
1^{9}	964	950
2	953	954
3	947	945
4	946	944
5	919	951
6	948	950
7	959	958
8	959	951
9	910	917
10	931	927
11	940	944
12	940	927
13	899	896
14	867	
14	893	906
15	901	915
16	927	920
17	944	940
18	946	941
19	958	958
	956	
20	925	934
20	926	924
21	932	936
	926	
22	935	934
22	913	
23	954	948
24^{1}	946	948
25	946	944
26	920	924
26	902	910

March.

Date	Oxygen.	
1	935	943
2	932	943
3	944	929
4	931	938
5	947	951
6	941	943
7	944	942
8	962	
9	001	006
10	946	954
11	911	
11	923	918
12	925	932
13	949	956
14	935	929
15	926	925
16	918	917
17	948	950
18	961	958
19	951	961
20	958	953
21	968	975
22	961	956
23	958	963
24	941	943
25	954	967
26	972	977
27	961	968
28	963	964
29	958	965
30	922	921
31	962	961

April.

Date.	Oxygen	
1	968	961
	968	
2	959	949
3	964	960
4	959	969
5	959	966
6	958	
7	949	951
8	972	963
	963	
9^{9}	941	940
10	984	985
11	976	972
12	971	969
13	965	
14	942	947
15	976	
16	950	954
17	960	963
18	967	962
19	965	964
20	960	960
21	959	970
22	959	965
23	963	961
24	958	967
25	964	965
26	963	965
27	970	968
28	963	950
29	943	940
30	939	947

May.

Date.	Oxygen.	
1	952	942
2	930	935
3	956	967
4	948	949
5	956	962
6	963	
7	955	954
8	977	
9	946	942
10	953	947
11	948	968
12	952	951
13	949	954
14	950	952
15	953	944
16	927	928
17	lost	
18	948	937
19	950	952
20	945	947
21	928	933
22	934	926
23	939	952
24	953	956
25	955	966
26	920	917
	920	
27	959	961
28	969	953
29	958	960
30	960	970
31	961	957

June.

Date.	Oxygen	
1	962	961
2	961	948
3	958	957
4	946	955
5	963	952
6	944	949
7	948	940
8	955	952
9	963	941
10	958	962
11	958	962
12	959	962
13	949	952
14	952	951
15	942	947
16	958	
17	935	951
18	935	930
19	950	949
20	948	961
21	955	959
22	945	940
23	958	960
24	955	947
25	948	962
26	957	947
27	958	956
	956	
28	911	908
29	952	947
30	952	962

October.

Date.	Oxygen.	
1	947	951
2	945	949
3	961	960
4	954	951
5	949	955
6	958	967
7	946	958
8	963	958
9	958	
10	951	954
11	961	960
12	949	961
13	957	959
14	963	966
15	956	960
16	970	964
17	956	970
18	972	963
	970	
19	956	969
20	961	974
21	954	969
22	961	948
23	962	967
24	962	972
25	948	952
26	967	956
27	866	868
	869	874
28	975	971
	980	976
29	981	969
30	930	936
31	952	947

November.

Date.	Oxygen	
1	969	979
2	951	949
3	969	962
4	968	963
5	984	969
6	967	976
7	951	952
8	954	969
9	976	963
10	960	973
11	956	965
12	964	963
13	964	964
14	965	
15	957	958
16	966	974
17	964	975
18	951	957
19	961	955
20	963	965
21	959	957
22	962	964
23	947	955
24	862	967
25	965	951
26	964	963
27	960	955
28	974	968
29	967	965
30	964	956

December.

Date	Oxygen.	
1	967	967
2	963	966
3	969	975
4	961	958
5	962	963
6	956	959
7	964	959
8	946	954
9	955	965
10	972	961
11	959	957
12	967	967
13	970	967
14	958	939
15	948	944
16	957	959
17	954	961
18	960	950
19	944	943
20	953	967
21	956	956
22	962	956
23	951	954
24	951	952
25	946	945
26	956	955
27	967	954
28	952	957
28^{8}	948	945
29	959	958
29^{8}	957	956
30	960	955
30^{8}	958	951
31	[illegible]	947

Oxygen found in the Air at Hudson, Ohio, from January to April, 1881.

January, 1881.		February.		March.		April.	
Date.	Oxygen.	Date.	Oxygen.	Date.	Oxygen.	Date.	Oxygen.
1	949 939	1	958 961	1	948 953	1	972 972
1[s]	936 940	2	959 953	2	950 956	2	974 975
2	954 960	3	954 960	3	967	3	965 964
2[s]	952 947	4	952 962	4	961 962	4	967 968
3	959 957	5	964 959	5	962 969	5	970 973
4	954 951	6	949 962	6	958 962	6	972 974
5	958 958	7	963 963	7	969 969	7	973 968
6	956 953		966 966	8	964 962	8	962 972
7	958 948	8	968 965	9	965 963	9	962 966
8	946 960	9	959 956	10	955 962	10	967 971
9	954 962	10	951 950	11	967 966	11	963 971
10	966 967	11	950 946	12	948 959	12	966 967
11	957 959	12	963 961	13	958 958	13	963 969
12	963 956	13	953 951	14	962 964	14	973 971
13	958 957	14	948 957	15	962 965	15	975 970
14	957 961	15	958 962	16	969 964	16	968 970
15	960 961	16	951	17	960 963	17	970 966
16	952 952	17	961 967	18	972 965	18	969
17	959 957	18	955 962	19[12]	960 960	19	964 960
18	950 952	19	959 961	20	956 954	20	969 963
19	956 956	20	957 954	21	974 974		
20	958 960	21	961 968	22	957 957		
21	948 950	22	950 954	23	970 970		
22	938 933	23	968 962	24	970 967		
23	955 947	24	963 972	25	967 969		
24	959 958	25	959 961	26	960 962		
25	952 947	26	958 959	27	962 963		
26	969 967	27	958	28	976 977		
27	959 959	28	943 950	29	952 955		
28	953 961			30	957 959		
29	960 954			31	973 972		
30	968 964						
31	960 962						

The figures in the column "Oxygen" are the 3d, 4th and 5th decimal places: before which are to be supplied the decimal point, followed by the figures 20. For example, 956 signifies 0·20956. But on March 9, 1880, supply 0·21. [12], collected at noon; [1], [2], [3], [4], [5], [8], [9], collected at 1, 2, 3, 4, 5, 8, 9, P. M.; [s], second sample collected nearly at the same time as the preceding sample.

winds at the stations of the Service all radiating from this center, especially if their velocities are considerable, we may infer with some fair probability that within this area of radiation there was a descent of air from an elevation.

I have, therefore, examined the Signal Service maps of the date of each deficiency in oxygen, or of each noteworthy fall in the amount of oxygen. The reproducing a score of these maps here would facilitate the forming an opinion as to the soundness of my hypothesis; but since the morning maps are easily accessible to most of those who would be interested in this

paper, and at no very long time after their date, I think it sufficient to describe what I learn from the examination, and to refer to the maps themselves those who may desire to form an independent opinion. Meanwhile I give, necessarily somewhat in detail, an account of the indications gained from my own comparison of the maps with the results of the analyses.

The analyses here tabulated have been made with the improved apparatus alluded to in a former article in this Journal as intended to lessen the probable error of an analysis. A comparison of the duplicate analyses made on the same sample will show that this intention has been carried out.

Jan. 9, 1880, 7 A. M.—There was an area of low pressure over the western part of Illinois, and of high pressure over the Gulf of St. Lawrence. The isobar of 30·10 inches ran from Montreal along the Appalachians to the Gulf of Mexico. East of the Appalachians, from Albany to near Knoxville, there were gentle winds of three or four miles an hour. They were not so directed as to indicate any general current across the mountains. West of the mountains there were brisk winds of eight miles an hour at Columbus, Cincinnati and Indianapolis, of nine miles at Pittsburgh, of ten miles at Buffalo and Louisville, and of from fifteen to twenty miles at Cleveland, Toledo and Erie. All these were nearly transverse to the axis of the Appalachians. A study of the weather map of this date suggests as a probable view that the mass of air passing to the northwest over Ohio was not part of a current passing at the surface of the earth over the Appalachians, but that it was in part the result of a descent of some upper current to the surface of the earth. Similar conditions continued at 3 P. M. At 2 P. M. the oxygen in the atmosphere at this place was 0·20936. It had been low on the preceding day, but observations of the wind and barometer were insufficient to determine the direction of the great currents of the air.

Jan. 10, 7 A. M.—There was an area of high barometer having its center over southwestern Ohio. From a little to the northwest of this center, winds were distinctly radiating in every direction with a velocity of about five miles an hour. It is therefore probable that the central area of high barometer was kept supplied by some current of air from an elevation. At 3 P. M. the center of radiating winds had moved so as to be over Lake Ontario. Cleveland was on the curve of 30·30 inches pressure. No winds were entering this curve at the surface of the earth as far as we can learn from the weather map of this hour, while they were moving outward all around it with a mean velocity of several miles an hour. The high pressure was not materially decreased by this outflow, and this continuance of high pressure was not due to rise of tempera-

ture for the region had become colder. It is, therefore, highly probable that some current of air from an elevation entered the area; and at 4 P. M. the oxygen found here was 0·20945.

Jan. 18, 3 P. M.—From the directions of the winds on the map of this date we could not be sure that the currents of air passing over Ohio had not come from the Gulf of Mexico after passing around a center of high pressure. But the maps for forty hours previously make it reasonably sure that no currents at the surface of the earth had brought air from the Gulf of Mexico to the vicinity of Cleveland, and so lead us to infer that the sample collected here at 4 P. M. was from air spreading out in all directions from the center of high pressure just mentioned, which was over the States of Mississippi and Tennessee. It contained 0·20939 oxygen.

The deficiency of oxygen found on January 21st is consistent with the theory proposed, but affords no evidence.

Jan. 26, 7 A. M.—There was an area of high barometer over the seaboard from Virginia to Maine. Over the southern part of Pennsylvania there was an obvious center of winds radiating in all directions with a mean velocity of about five miles an hour. It is probable that the withdrawal of air from this area was made good by the entrance into it of air from some upper current. At 9 A. M. the amount of oxygen in the air at this place was 0·20931.

Jan. 29, 3 P. M.—At this time there was an area of high barometer having its center a little to the northwest of Montreal. The same point seems also to have been a center from which winds radiated in every direction. Although there are no stations reporting from the northern half of the supposed circle, a circular course of isobars is clearly indicated; the winds to the east of the center blow to the east, and those to the west of the center blow due west. The mean velocity of winds passing out from the southern half of the assumed circle is ten or twelve miles an hour. If, then, there was a closed curve to the north, where the air was also passing out, there must have been a rather rapid supply of air brought into this area by currents from an elevation, and at 9 A. M. the oxygen found at this place was 0·20926.

Feb. 9, 7 A. M.—There was at this time an area of high barometer within which the isobar of 30·40 inches was nearly a circle having Lake Michigan for its center. Stations are but few to the north and northwest of this circle, so that it cannot be absolutely affirmed that winds were blowing outward all around the circumference. But the gentleness of the winds over the northern half of the circle, and the briskness of winds passing over its southern semi-circumference, point to a descent of upper currents. The oxygen found here at 9 A. M. was 0·20914.

Feb. 10, 7 A. M.—High pressure now prevailed off the coast of New England. The form of the isobars and the directions of the winds reported show a gentle current of air along the eastern slope of the Appalachians, but no motion across them. The crowding together of isobars running along the axis of the mountains, as well as the uniform northwest motion of the winds over an area reaching from Cincinnati to Oswego and Kingston, make it probable that air from an upper current came down to the surface of the earth near lake Erie; and the oxygen found here at 9 A. M. was 0·20929.

Feb. 11, 7 A. M.—At this hour there was an area of high barometer over Chesapeake Bay. The winds were all moving away from the Appalachians, both on the eastern and the western slopes. It is therefore likely that there was a descent of air from some upper current. The oxygen found here at 9 A. M. was 0·20942.

Feb. 12, 7 A. M.—There was now an area of high barometer off the coast of South Carolina. East of the Appalachians most of the winds were parallel to the axis of the chain. West of the mountains the winds may well enough have come from the Gulf of Mexico, but the acceleration of the winds as we look toward the center of low pressure near the upper lakes may perhaps indicate a descent of upper currents. The oxygen found at 9 A. M. was 0·20934.

On Feb. 13th the oxygen found was 0·20897; this case needs special discussion.

Feb. 20, at 7 A. M.—There was an area of high pressure having its center near Pittsburgh. Winds radiated in all directions from this center. The inference of the descent of upper currents is probable. At 9 A. M. the amount of oxygen found here was 0·20930, and in another sample taken at nearly the same time, 0·20925.

At the two hours of observation next following the one just mentioned, the same divergence of winds from a center seems to have continued, the center passing slowly to the east. Now at 7 A. M. on the 21st, no such condition of radiating winds continued, but the oxygen found at 9 A. M. was 0·20926. Possibly the long continuance of conditions favoring the descent of upper currents had brought to the surface of the earth near this place a volume of air poor in oxygen which had not yet been carried away under succeeding conditions.

February 22d.—At 7 A. M. there was an area of high barometer a little to the south of the Ohio River. At 9 A. M. the oxygen found here was 0·20934, and in another sample 0·20913.

A deficiency of oxygen was also observed on February 26th, and on March 11th, 12th and 15th, but I am able to suggest no satisfactory explanation of either case.

March 24th.—At 7 A. M. there was an area of high pressure having its center at Lake Superior. Winds were diverging all around the southern half of a circle; no stations were far enough to the north to give any certain knowledge as to their direction over the northern half. The oxygen found here at 9 A. M. was 0·20942.

The same remark may be made as to March 30th. A center of high pressure existed near Lake Superior, and the winds radiated from the southern half of a circle. The oxygen found at 9 A. M. was 0·20922.

April 9th.—At 7 A. M. there was an area of high barometer reaching from Texas to Tennessee. North of latitude 37 degrees, and east of St. Louis, almost every reported wind was blowing toward the north or northeast, while on the south of the same line the winds reported were all blowing toward the south or southwest. We may therefore suppose that some descent of upper currents would occur; at 9 A. M. the oxygen in the air here was 0·20940.

April 14th.—At 7 A. M. there was a center of high pressure off the coast of Georgia, but the data are too incomplete for trustworthy inference. The oxygen found at 9 A. M. was 0·20945. On the 16th, the data are also too incomplete.

April 28th.—At 1 A. M. there was an area of high pressure with distinctly radiating winds having their center on the Ohio River. At 7 A. M. this center was over West Virginia. At 9 A. M. the oxygen found in the air here was 0·20957, which was a fall of 0·00012. The cause continued to operate for some time afterwards, and on the next day the oxygen found was 0·20941. On the 30th, in spite of the passage of an area of low barometer the oxygen continued low, being found to be 0·20943. On May 1st there was a center of radiating winds over the southeast part of Kentucky. Under the influence of the descent of upper air which probably occurred, the oxygen found at 9 A. M. was 0·20947.

On the 2d this center of radiating winds was over North Carolina, and the oxygen found here was 0·20932 at 9 A. M.

On the 9th and 10th an area of high barometer hovered over the sea coast of North Carolina for twenty-four hours, with well marked diverging winds over the observed half of the circumference of a circle. On the 9th the oxygen found here was 0·20944, and on the 10th it was 0·20950, both at 9 A. M. An area of high pressure now developed near Lake Michigan and hovered to the north of the lower lakes till the morning of the 16th, when it passed over the lakes toward South Carolina. During these days the oxygen found here at 9 A. M. was 0·20950, 0·20953, 0·20951, 0·20951, and 0·20948, and on the 16th when the area of high pressure became central near Cleveland, the oxygen found at 9 A. M. was 0.20927.

I cannot suggest an explanation of the deficiency in oxygen which occurred May 21st and 22d.

May 26th.—At 7 A. M. there was an area of high pressure over Georgia, with winds spreading in all directions around the landward side of a circle. At 9 A. M. the oxygen found was 0·20919.

June 17th.—At 7 A. M. there was a large area of slight excess of pressure covering most of the Northwestern States. On the 18th at 7 A. M. the high pressure had moved to the east, and the highest pressure observed was at Cleveland. Under its influence, the amount of oxygen found at 9 A. M. was 0·20933.

The deficiency of oxygen observed on June 28th I am not able to explain; the data being insufficient.

Having made my application too late, I have not obtained a series of the thrice-daily weather maps for comparison with my observations on variations in amount of oxygen for a period of six and two-thirds months beginning with October, 1880. A comparison of my observations with the daily morning maps leaves some facts unexplained which the possession of fuller data might clear up satisfactorily. All the maps used being of the seven o'clock series, it will not be needful to specify the hour further; the observations of oxygen were all made at the same time with the observations from which the maps were made.

I cannot explain the deficiency of oxygen occurring on October 4th.

On October 5th there was a long narrow area of high pressure reaching from New England to Texas. The spreading out of the air on each side of this area was well marked, the inference that there was a descent of upper currents to the surface is well sustained, and the oxygen found was 0·20952.

October 7th. An area of high pressure had its center near Lake Erie, with winds spreading outward in all directions. The oxygen found was 0·20952, a fall of 0·00010.

October 10th.—A long narrow area of high pressure extended from Maine to Texas. The radiation of winds in all directions was decided, and the inference that downward currents mingled the surface air with air from an elevation reasonably probable. The oxygen found here was 0·20953. But this was a fall of only 0·00007, which is a pretty small difference. The probability that there really was a fall in the amount of oxygen is only about six to one.

The deficiency of oxygen on October 22d I cannot explain.

On the 25th, winds were radiating in all directions from an area of high pressure in Eastern Tennessee. The oxygen found was 0·20950, a fall of 0·00017 from the previous day.

The chances that there was really a fall are here about 8000 to 1.

October 27.—There was an area of high barometer with its center well to the north near Lake Superior. Over the observed half circumference of a circle, the winds were distinctly spreading in all directions. The inference that there were downward currents of air is also supported by the fact of an apparent excess of velocity in the winds passing south over the lakes over the velocity of winds approaching them from the north. Under the influence of this or some other cause, the oxygen found here was 0·20869.

I cannot explain the deficiency of oxygen on October 30th.

On November 2d there was an area of high pressure with its center over Tennessee and North Carolina. In a general way, winds diverged from Kentucky and West Virginia. But there were counter currents, so that with the materials at hand we are not authorized to say that a descent of upper currents is indicated with any such probability as to explain the deficiency of oxygen found. The amount was 0·20950.

November 7th.—There was an area of high pressure with its center over Mississippi. From this winds diverged all around the northern half of a circle. There were some local winds in Upper Lake Region which did not conform to this system; but the shape of the isobars makes it fairly probable that in the region of the lower lakes there were descending currents of air from the upper part of the atmosphere. The oxygen found was 0·20951, a fall of 0·00020 since the day before.

November 18th.—There was an area of high pressure over the Indian Territory. From this area winds were spreading outward over the observed half circle of stations. An acceleration of the winds in the Lower Lake Regions suggests a supply of air from above to feed these winds. On the 19th this area of high pressure had its center in Southern Ohio. The inference of a descent of upper currents is as clear as it can be from maps with no more reporting points than those now established. The oxygen found was 0·20958, a fall of 0·00010 since the 17th. On the 18th it had been 0·20954.

On November 23d there was an area of high pressure with its center over Southern Ohio, and the inference that upper currents reinforced the winds which spread out all around this area is a probable one. The oxygen found was 0·20951, a fall of 0·00012 from the preceding day.

December 3.—At this time there was an area of high pressure with its center over Southern Ohio. It had moved with great velocity from over the Upper Missouri Region; winds were diverging from a center over Southwestern Ohio. The oxygen found was not affected. On the 4th the area of high

barometer was central over Chesapeake Bay, and the winds blowing toward the northwest from the Appalachian Mountains seemed not to blow over them from the east. The oxygen now found was 0·20960, which was a fall of 0·00012 from the preceding day.

The deficiency of oxygen on the 8th I cannot explain satisfactorily.

On December 14th there was a high barometer off the coast of Florida. As often happens, the Appalachians seemed to act as a barrier. The winds blowing from them toward the northwest had nothing to do with the winds to the east of the mountains. If we may thence infer a descent of upper currents in this region, we shall account for the fall in the amount of oxygen, which amounted to 0·00010.

The deficiency in oxygen noticed the next day is easily explained according to the working hypothesis suggested, but does not add to the evidence for it. The same is true of the deficiency observed on the 19th.

On December 23d, a very wide area of high pressure with winds diverging from the area affords a reasonable presumption that these winds were reinforced by a descent of upper currents. The oxygen found was 0·20953, a fall of 0·00006, which is too small for safe deduction. On the 24th and 25th the center of high pressure was nearly stationary over the lower St. Lawrence. The directions of the winds are rather confused, but distinctly exhibit the tendency according to which winds radiating from a common center of high pressure are likely to be accompanied by a deficiency of oxygen. The oxygen found was 0·20951 and 0·20945 respectively on the two days.

The rapid fall in the amount of oxygen on the 28th affords no evidence for or against the theory.

The deficiency of oxygen on December 31st and January 1st was probably due to the occurrence of an area of high pressure over the Appalachian Mountains on each of those days. From each side of this area winds were blowing outward. The amount of oxygen on the 31st was 0·20951; on the 1st it was 0·20944 in the morning, and 0·20938 in the evening. From the weather map of the 2d, according to my theory it would be expected that there would be a deficiency of oxygen; but while the evening observation showed a slight deficiency, the morning observation showed none.

The map for the 22d gives reports from but a few stations, so that the data are too few for trustworthy inference. As far as the facts go, they seem little conformed to the theory.

January 25th.—At this date there was an area of high pressure over Mississippi. Winds were spreading outward in all directions around this area, the oxygen found was 0·20949.

The deficiency of oxygen noticed on the 28th was well explained by the occurrence of an area of high pressure over Missouri, with winds radiating around it. On the 29th, this area was central over eastern Tennessee, the winds well exhibited the spreading out in all directions which suggests the descent of upper currents. The oxygen found on these days was 0·20957, a fall of 0·00011 as compared with the 26th.

The morning of February 9th affords a reasonably clear proof that the surface winds implied the descent of upper currents. There was an area of low pressure over the mouth of the Mississippi, and one of high pressure on the northern half of the Atlantic seaboard. East of the Appalachians, no winds were directed across the mountains, while on the western side of the mountains, from Louisville to Montreal, the winds were all radiating from a center in Pennsylvania, with a mean velocity of eight miles an hour. On the next day the stations near Louisville were involved in currents coming from the gulf, and gentle winds were blowing from the seaboard toward the Appalachians. There was an area of low pressure in Michigan. The obvious acceleration of the winds in the lower lake region suggests a continuance of the descent of upper currents which probably occurred on the 9th. The oxygen observed on these days was 0·20958 and 0·20951, against 0·20967 on the 8th. The deficiency of oxygen continued till the 11th, with an area of high pressure reaching from eastern Pennsylvania over the lake region to the northwest. The oxygen now observed fell to 0·20948.

There was a deficiency of oxygen on the 13th. There was a general brisk motion of winds toward a center of low pressure in Maine, with nothing explaining the observed deficiency.

On the 15th, there was an area of high pressure with its center over New Jersey. Winds blew away from this center in every direction. No deficiency of oxygen was observed, however, on the morning of this day, but on the next day the oxygen found was 0·20951, a fall of 0·00009. Nothing on the maps of this morning explains this deficiency.

On February 21st, there was an area of high barometer over the lower Mississippi valley. Winds were blowing outward in all directions. The inference that there was a descent of upper currents is perhaps a fairly probable one. The oxygen was not affected at this place, being found to be 0·20964. On the 22d there was an area of low pressure over Lake Superior, toward which winds were drawn with increasing velocity from the northwest slope of the Appalachians, while on the other side of the mountains the winds show no connection with the system prevailing on the northwest side. Over the region from Louisville to Kingston the mean velocity of the winds was

eleven miles an hour. It seems almost certain that there must have been a descent of upper currents. The oxygen found on this morning was 0·20952, a fall of 0·00012.

On February 28th, the oxygen here fell to 0·20947, but there is nothing in the map of that date suggesting any explanation. On the previous day, however, there were such conditions as seem to indicate that a deficiency of oxygen was to be expected. If from other evidence my theory should seem to be a first approximation to some law of nature, it will be supposed that some air deficient in oxygen, brought to the surface of the earth by the conditions prevailing on the 27th, came to the observer here on the next day. But this is almost too precarious to be mentioned.

On March 1st and 2d, there was an area of high pressure over Lake Superior, with winds radiating around the observed third part of a circle. The oxygen found on these days was 0·20951 and 0·20953. On the 3d, there was an area of low pressure and a storm of considerable violence over the Ohio valley, and the oxygen found promptly went up to 0·20967.

On March 12th, there was an area of relatively high pressure over Lake Ontario, with a storm having its center of low pressure in Kansas. Winds were directed away from Lake Ontario in all directions, the inference that there occurred a descent of upper currents is a reasonable one, and the oxygen found was 0·20954, a fall of 0·00012.

March 19th.—At this time there was an area of high pressure in Maine, and of low pressure over western Kentucky. The barometer was three-tenths lower at Cincinnati and four-tenths lower at Louisville than it was at Cleveland, with this high barometric gradient, the winds were much accelerated in passing over Cleveland toward the southwest, and it may be that upper currents were compelled to descend and mingle with them. The oxygen found was 0·20960, a fall of 0·00008.

On March 20th, there was an area of low pressure just east of Lake Michigan. The deficiency of oxygen is not explained by the weather map. The same is also true of the 29th and 30th.

As far as I can see, it is impossible to discern any connection between the deficiencies of oxygen observed, and the direction of the wind at the time of taking the sample.

My own judgment, from the comparison detailed, is, that the theory that deficiencies in the amount of oxygen in the atmosphere are caused by the descent of air from an elevation fairly well agrees with the facts.

ART. LV.—*On Jolly's Hypothesis as to the Cause of the Variations in the Proportion of Oxygen in the Atmosphere;* by EDWARD W. MORLEY, M.D., Ph.D., Hurlbut Professor of Chemistry in Western Reserve College.

JOLLY has suggested a certain hypothesis as to the cause of those variations in the ratio of oxygen to nitrogen which are from time to time observed in the atmosphere of a given place. He supposes that the volumes of air which exhibit the deficiency of oxygen are brought by currents from the tropical regions, that the deficiency of oxygen was caused in those regions, that it was caused by the consumption of oxygen in the oxidation of organic matter, and that at some places within the tropics this consumption is therefore considerably greater than the liberation of oxygen in the processes of vegetation.

I have proposed a second hypothesis. I suppose that the volumes of air deficient in oxygen are brought by currents from an elevation above the surface of the earth, that the deficiency of oxygen was caused while these volumes were at this elevation, and that it was caused by that assumed physical action according to which, in a high vertical column of a mixture of two gases, the heavier will tend to become less abundant at the top of the column.

The labor of establishing either hypothesis by experiment will probably be considerable. I propose to mention some reasons which seem to indicate that Jolly's hypothesis is the less probable.

1. There is no direct evidence that the atmosphere near the equator is poorer in oxygen than the air of higher latitudes. Numerous analyses agree in this result. Lewy's analyses of air, collected at Guadeloupe, show that the mean ratio of oxygen to nitrogen there is the same as that at Paris.

2. It is difficult to ascribe to the cause assumed by Jolly a magnitude sufficient to produce the observed effect.

If a volume of air at latitude fifty degrees is deficient in oxygen by 0·004 or 0·005, the deficiency must have originally been far greater, if this air has come from the tropics, and has thus for many hundreds of miles been exposed to admixture with normal air. We must either, in the first place, believe that at some parts of the tropical regions there are not very seldom immense volumes of air, deficient in oxygen to the amount of 0·01 or more; or, in the second place, we must assert that the analyses which show deficiencies of oxygen at latitude fifty degrees amounting to 0·004 or 0·005 are grossly in error, and that the actual deficiencies are very much less; or, in the third place, we must abandon the hypothesis. If the analyses are

trustworthy we must abandon the hypothesis, or else attribute to its supposed cause a magnitude altogether incredible. I will examine some of the experimental evidence, that the oxygen in the atmosphere sometimes falls below the mean by as much as 0·004 or 0·005.

I will not cite any analyses made before the year 1841. In that year Dumas and Boussingault found it necessary to resolve by experiment the doubt as to whether the true proportion of oxygen in the air were exactly one-fifth, or were about twenty-one per cent, or were a variable quantity. If this was the uncertainty as to the mean of multitudes of analyses, it is obvious that we can by no means attribute to a single analysis a degree of precision sufficient to aid in the present inquiry. But in that year, Dumas and Boussingault used a new method of analysis, by means of which sufficient accuracy was obtained, and proposed an elaborate system of analyses on air collected simultaneously at different places. Lewy went to Copenhagen to take part in this system, carrying with him apparatus from the laboratory of Dumas and Boussingault. He had the coöperation of Oersted, and his results were communicated to the Academie des Sciences by Dumas and Boussingault. Four of his results on four samples of air, collected at sea on the voyage to Copenhagen, showed a proportion of oxygen as low as 0·2045.

Regnault's results will command entire confidence. A sample collected in the Bay of Algiers, June 5, 1851, gave 0·2042 and 0·2040 oxygen. A sample collected in the Bay of Bengal, February 1, 1849, gave 0·2046 and 0·2045 oxygen.

Jolly has used a new method equally accurate with the common process, by explosion with hydrogen, and very valuable as confirming the latter. He measures the tension of a confined volume of air while it is at the freezing-point. He then absorbs the oxygen from this air by means of a copper spiral heated by electricity, and again measures the tension at the freezing-point. The absorption and measurement are repeated till no more absorption takes place. A sample of air collected at Munich, June 15, 1877, gave 0·2053 oxygen, one collected July 19, gave 0·2056, and one collected November 10, gave 0·2056. Also at six other dates during the same months he found the amount of oxygen in the air less than 0·207.

A sample taken at this place, September 20, 1878, gave 0·2049 and 0·2049 oxygen. A sample taken February 26, 1879, gave 0·2045. The other analysis of this sample was lost by the accidental use of hydrogen containing a little air. But even this analysis, which of course gave the proportion of oxygen too high, gave only 0·2049.

The analyses of Macagno, at Palermo, made by absorbing oxygen with pyrogallol, I forbear to cite.

It is difficult to resist the conclusion that these analyses show that sometimes the deficiency of oxygen observed in the atmosphere at such latitudes as fifty-two, forty-eight, and forty-two degrees, may amount to 0·004 or 0·005. Then we must either suppose that not very seldom there might be observed within the tropics immense volumes of air in which the deficiency should be several times as great as this, or we must abandon the hypothesis in question.

If processes of oxidation preponderate over processes of reduction within the tropics, there must be a transportation of organic matter from colder climates toward the equator, there to be oxidized, but

3. No such amount of transportation as is required by the hypothesis takes place through the air. For, in the first place, experiment has repeatedly shown that after a volume of air has been freed from carbonic acid, there is left in it but a minute trace of matter capable of undergoing oxidation. Now, if a given volume of air contained an amount of organic matter capable, in its oxidation, of absorbing from this volume of air 0·005 of oxygen; and if further this organic matter was as rich in hydrogen as is marsh gas, even then the carbonic acid produced in some of these experiments would have been ten times as much as the carbonic acid already existing in the air. And secondly, if the observed deficiency of oxygen in the atmosphere had been produced by the oxidation of organic matter previously contained in it, the missing oxygen would be replaced in part by the carbonic acid produced, which, on the most favorable assumption, would amount to half the deficiency of oxygen. But experiment has shown that no such amount of carbonic acid is ever found in air uncontaminated by local causes, though a very large number of determinations has been made.

4. The transportation of organic matter required by the theory does not take place by the waters of the globe. If Jolly's hypothesis is true a very large part of the organic matter returned to the air in the form of carbonic acid must be supposed to be dissolved or suspended in the water which flows from the land into the sea, to be brought by ocean currents to the equatorial parts of the ocean, and there to be at last oxidized.

It may be noticed that this supposition would permit us to explain the removal of oxygen from the air without the restoration of a corresponding volume of carbonic acid to the same volume of air, by assuming that the oxidation takes place in the waters of the ocean while near the equator, but that the carbonic acid there produced is restored to the air but slowly, and therefore is not restored to the volume of air which afforded the oxygen.

Now, if this supposed mechanism of oxidation is not consistent with observed facts, the theory that the atmosphere within the tropics sometimes shows a deficiency of oxygen produced by the preponderance of processes of oxidation over those of reduction must be dismissed from consideration. My own knowledge is far from sufficient to enable me to assert that the hypothesis is disproved by facts already observed. But I may mention some of the points in which the theory may be compared with facts capable of easy observation, or perhaps already observed. Those who are familiar with observations on the chemistry of sea-water will be able to judge whether the hypothesis is not overthrown by these facts thus compared.

In the first place, if the supposed process of oxidation is the actual process, it must obviously be about as regular and invariable as the motion of rivers and ocean currents. A vigorous withdrawal of oxygen from the superincumbent air must then go on constantly within certain areas of the ocean. Whenever a volume of air is becalmed over such an area, so that the cause may operate for some time on the same air, such air should be highly deficient in oxygen. Now, can we find any evidence that air over some parts of the tropical oceans is specially deficient in oxygen whenever the winds are slight? If the evidence is of the opposite nature, Jolly's hypothesis lacks confirmation.

Again if the supposed oxidation takes place in the water, a somewhat rapid transfer of oxygen must go on between the air and the water. In the regions in question, whenever the sea is still, then there must be a falling off in the quantity of oxygen at different depths in the ocean. The contrast in this respect between equatorial waters and those at forty-five degrees of latitude ought to be capable of observation. A collation of results already obtained may perhaps afford a decisive test of the theory.

In the third place, if the supposed oxidation takes place through the waters of the sea, the retention of the carbonic acid produced is somewhat protracted. Determinations of carbonic acid in the air are very numerous, but no observer has yet found normal air containing one or two hundredths per cent of carbonic acid more than the average. Then, even when air is exposed long enough to oxygen-absorbing water to lose 0·005 of oxygen, it does not gain a noteworthy amount of carbonic acid. Now if the carbonic acid produced is thus retained, the water of some parts of the equatorial seas must be very abundant in carbonic acid. There must be a gradual diminution toward the poles; and further, within all moderate latitudes, there can be no equilibrium between the tension of carbonic acid in the air and that of carbonic acid in sea-

water. If facts do not agree with these deductions, the supposition that a large part of the processes of oxidation on the surface of the globe takes place in sea-water within the tropics is contrary to the facts.

In the fourth place, it is doubtful whether rivers carry any such amount of organic matter as is required by the theory. Determinations of the amount of oxidizable matter contained in the water of rivers have been chiefly limited to the water supply of towns. But some observations have been made on the water of the Nile. Tidy found by the permanganate process that 0·23 grain of oxygen was given up to a gallon of the water of this river. If we take this result to represent the amount of oxygen absorbed by river water *after the water reaches the tropics*, we shall concede much for argument. Such water could remove 0·001 oxygen from about ten times its own volume of air. Of course it is difficult to suppose that the consumption of oxygen can be localized in a small volume of air. Now, if such water be diluted with sea-water, and if it absorbs oxygen from a hundred times its volume of air, through several degrees of latitude, and if the deficiency of oxygen to be explained is several times 0·001, it is hard to believe that the cause is sufficient.

5. It is very doubtful whether the whole consumption of oxygen on the globe would account for the observed deficiencies of oxygen, even if we suppose this total consumption for a certain short period to be taken from one and the same small volume of air.

Dumas and Boussingault made an approximate estimate of the amount of oxygen used in a century by all process of oxidation. If we take this estimate we shall find that all the oxygen absorbed from the air in a week, if taken from the same volume of air covering but half a square degree of the earth's surface, and containing only the lower third part of the atmosphere, would produce in this limited volume a deficiency of oxygen of but one-eighth of one per cent. But we have to account for deficiencies several times as large, and we cannot suppose the consumption so limited to a small volume. Then the theory fails to agree with the facts.

At the foundation of the hypothesis which I have suggested to account for the observed deficiencies in the oxygen of the atmosphere, there lies the assumption that in a vertical column of a mixture of two gases of different densities, there is a tendency to the accumulation of a greater proportion of the heavier gas toward the bottom, and of a greater proportion of the lighter toward the top. There has not yet been obtained any direct experimental evidence in favor of this theory of

Dalton. Although the assumption is a simple, and, I think, certain inference from the known principles of mechanics as applied to gases, it is desirable that experimental evidence should be supplied. I have planned two forms of apparatus and two series of experiments for this purpose; but the making of a more perfect eudiometric apparatus than had heretofore been used, the carrying on a series of daily analyses in duplicate of samples of air collected at this place, and the providing for the collection of samples at other parts of the continent, have used so much of my time and income that so far it has been impossible to carry out these plans. I hope before long to supply this deficiency.

ART. LVI.—*Lower Silurian Fossils in Northern Maine;* by W. W. DODGE.

THE writer found graptolites in black shale in No. 3 township, of Range VII, Penobscot county, Maine, in September last. The fossils are, for the most part, mere bright films upon the dark rock, and in the small quantity of material brought away, but one or two individuals are sufficiently distinct and entire for identification. The fragments are of at least four varieties; the *Diplograptus* type predominates.

The most complete specimen is one of *Diplograptus pristis*, but of this the upper end of the axis is broken away. The cellules are about sixteen to an inch in each rank. Instead of narrowing gradually from end to end, as the drawings usually represent, the stipe retains its full width for an inch and a half and then its edges approach each other rapidly in the next half inch toward the solid, acicular radicle.

A clearly-marked fragment, three-eighths of an inch long, is of a width only half that of the preceding, the axis is much more distinct, the cellules, twenty-four to an inch on each side, although separated from one another nearly to the base by a rounded interval of about one-third their own width, are so shaped, with the denticle turned inward, that the appearance of serration in the stipe is subordinate to its linear, parallel-edged aspect. The general shape of what is visible is suggestive of *Graptolithus ramosus*, although no bifurcation appears. Close beside this is a branching fragment upon which no cellules are discernible, probably its stem.

One or two small, broadly-ovate shapes, perhaps *Phyllograptus*, and a few long, slender stems not sufficiently characteristic, or too incomplete, for their relations to be ascertainable, conclude the list of forms at present in hand.

The shale in which these remains are embedded is probably

to be referred to the level of the Utica slate or the Hudson River formation.

The locality is on the north side of the Wassatiquoik River, about a mile west of the East Branch of the Penobscot. The road to Katahdin Lake crosses the southern slope of Wassatiquoik Mountain (the eastern and smaller of the two so-called, the one which stands in Range VII upon the line between Nos. 3 and 4), while the river of that name runs at its foot. The shale is at the base of the hill on the eastern side—under its lee, with reference to glacial erosion.

The occurrence of fossiliferous rocks here is interesting as helping to correlate the Maine formations with the better understood Canadian strata, and also as narrowing the circle of known fossil-bearing beds about the Katahdin granite, whose position and age may sometime be determined by its relations to them, when a point of contact is found. Graptolites have been found in New Brunswick in that great belt of strata mapped as extending southwestward from the Bay of Chaleurs, with granite bands on its southeast side.*

The readiest cleavage of the thinly-layered shale which holds the above described fossils, is at 30° across the plane in which they lie. There is noticeable uniformity in the position of the long, slender forms, but the means is not at hand of determining through how great a thickness of accumulating strata the parallelism continued. The rock most nearly associated with the black shale is a black, or dark-blue, very hard, thick-bedded slate, of conchoidal fracture, sometimes semi-translucent in thin flakes. Another rock was too deeply weathered for examination with such tools as could be improvised. A coarse "greenstone" forms a ledge near by; and the presence of intrusives doubtless accounts for the condition of the flinty-looking slates. The only rock noticed in the three miles to the westward is a dull, greenish, hydrous-looking eruptive, mostly in boulders. Water-worn pebbles in the vicinity, apparently of this kind, are streaked with dull red, and show many cavities.

The nearest observed outcrop to the eastward is of slate with an easterly dip, on the left bank of the East Branch, near the water at its summer level, about opposite the mouth of the Wassatiquoik River. This is a mile and a half north of the Hunt farm, two miles east of which the road crosses a slate ledge where the strata dip to the westward. The outcrops of this slate along the East Branch have been examined by different observers, and its strike and dip at many points recorded.†

* J. W. Dawson, Acadian Geology, 1878, supplement to second edition, p. 78.

† C. T. Jackson, Second Annual Report on the Geology of the Public Lands of Maine and Massachusetts, 1838, pp. 20–24; C. H. Hitchcock, Agric. and Geol. Maine, 1861, pp. 392. 393.

One of the most noticeable facts connected with the presence of this rock between Molunkus and Sherman, along the post-road from Mattawamkeag to Patten, is the large amount of clear-white, fine-grained quartz rock scattered by the roadside.

The road from Sherman (No. 3, of Range V), to the East Branch at the Hunt farm, gives a good line of section nearly at a right angle across the line of strike there prevalent, and by comparison of the dips near the road and elsewhere, it seems to cross not less than four anticlinals and five synclinals. The western portion of the road is through woods. There is a large exposure of nearly vertical beds on the west side of Swift Brook. Between the brook and Sherman, a distance of five miles through partially cleared country, the road crosses four long ridges of high land, whose direction is that of the strike of the underlying rocks. Upon the hills the strata crop out occasionally, and in the valleys between flow small streams at regular intervals of a little over a mile from each other. On the hill just south of the village of Sherman, and near the line between Nos. 2 and 3, the slate shows a high dip westward.

Glacial.—The parallel courses to which so many of the long, narrow lakes and large and small streams of the northern part of Maine conform, appear to indicate the undeviating direction of primary glacial erosion in that region. The course of transported bowlders agrees well with this, as in the case of the limestone *in situ* in No. 4, R. IX,* observed in scattered bowlders upon the Wassatiquoik and at Whetstone Falls, on the East Branch, in No. 2, R. VII.† The uniform shaping of resistant ledges, such as may be seen at Mt. Kineo, and as is recorded of the slates along Webster Stream and at Grand Lake,‡ indicates in a general way the direction of the force exerted. The glacial striæ, as reported, appear to be more than usually divergent. To the two localities of the occurrence of granite bowlders from an unknown source named by Professor Hitchcock—north end of Churchill Lake in No. 9 of R. XII,§ and No. 5 of R. VIII,‖—may be added the site of one, high on the hillside above the East Branch opposite the Wassatiquoik. The granite pebbles in the bed of the Wassatiquoik at the dam, four miles above its mouth, may belong to the Katahdin mass, but the extent of the area occupied by this has not been definitely determined. The "porphyry" on Soper Brook,¶ in No. 8 of R. XII, may well be the source of the pebbles of porphyritic black felsite with quartz grains found at this dam.

* Agric. and Geol. Me., 1862, p. 321. † Ib. 1861, p. 393.
‡ Thoreau, Maine Woods, pp. 262, 277. § Agric. and Geol. Me., 1861, p. 411.
‖ Ib. p. 401. ¶ Ib. p. 411.

Cambridge, Mass.

ART. LVII.—*A Contribution to Croll's Theory of Secular Climatal Changes;** by W. J. McGEE, of Farley, Iowa.

BRIEFLY stated, Dr. Croll's theory of secular changes in terrestrial climate indicates that during periods of high eccentricity in the earth's orbit the hemisphere whose winters occur in aphelion suffers, through the intervention of physical and meteorological agencies, a diminution of temperature, while on the opposite hemisphere the temperature is correspondingly augmented.† It is the object of the present communication to direct attention to certain meteorological relations tending to produce such an effect, which appear to have been heretofore overlooked.

All extensive series of meteorological observations which have been examined by the writer indicate the existence of a general law, which may be expressed by the proposition: *Any increase in annual or diurnal thermometrical range is accompanied by a diminution in mean temperature.* Aside from the collocation of a portion of the results of an elaborate meteorological survey, for the purpose of establishing an empirical coefficient indicating the efficiency of the law in absolute measure, no discussion of this proposition will be here offered.‡

The accompanying table I is based upon Charte III of W. H. Dove's "Verbreitung der Wärme auf der Oberfläche der Erde,"§ and exhibits temperatures along the meridians passing through the Atlantic Ocean (long. 20° W.) and Central Asia (long. 120° E.).

TABLE I.

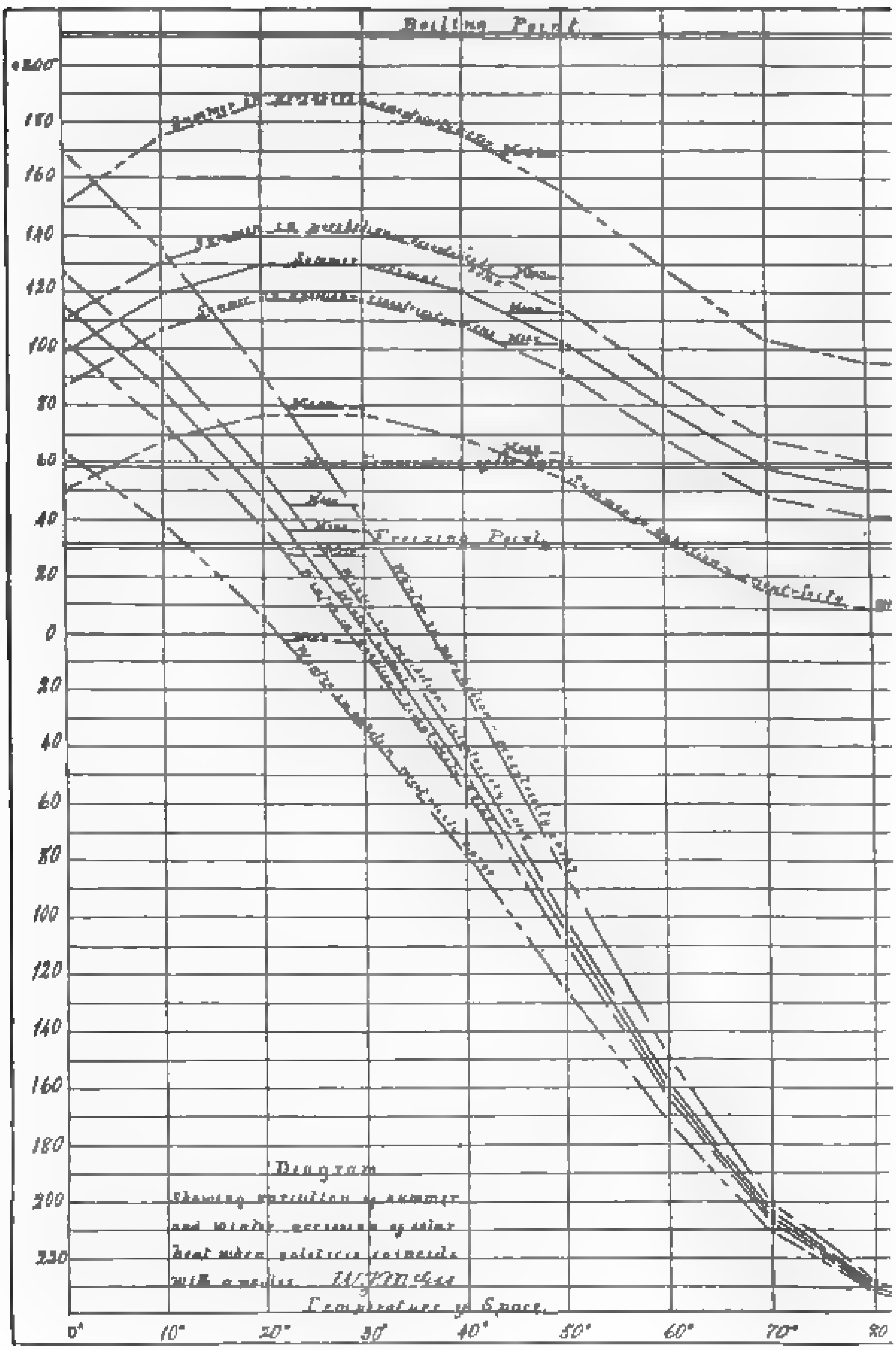
Temperatures at 20° W. and 120° E. of Greenwich.

Latitude.	Long. 20° W.			Long. 120° E.		
	January.	July.	Range.	January.	July.	Range.
0°	+79·2°	+77·0°	− 2·2°	+77·0°	+81·5°	4·5°
20	70·0	77·0	+ 7·0	60·6	81·5	20·9
40	55·9	68·0	12·1	26·4	75·4	49·0
60	36·0	57·2	21·2	−34·0	63·5	97·5
Polar circle	26·2	45·5	19·2	40·0	60·1	100·1
Mean	68·4	73·3	4·9	53·4	78·8	25·4

* Read before the Iowa Academy of Sciences, June 25th, 1880, and printed in brief abstract in the Proceedings, vol. i, pt. 1, p. 24. Read before the American Association for the Advancement of Science at Cincinnati, August 22d, 1881.

† Lond., Edinb. and Dublin Phil. Mag., Aug., 1864, Feb., 1867, etc. "Climate and Time." Edinburgh and New York, 1875.

‡ Cf. Proceedings American Association, vol. xxix, Boston Meeting, 1880, p. 486, *et seq.* § Berlin, 1852.


Boiling Point.
Freezing Point.
Mean Temperature of the Earth
Diagram
Showing variation of summer
and winter accession of solar
heat when solstices coincide
Temperature of Space.
+200°
180
160
140
120
100
80
60
40
20
0
20
40
60
80
100
120
140
160
180
200
220
0°
10°
20°
30°
40°
50°
60°
70°
80

In computing the means the following coefficients were employed:

Latitude.	Coefficient.
0°	1·000
20	·658
40	·357
60	·134
Polar circle	·074

The mean annual temperatures are 70·8° and 66·1° respectively. It thus appears that the mean temperature is 4·7° lower and the thermometrical range 20·5° greater over the land-meridian than over the water-meridian; which ratio yields a coefficient of diminution of 0·23° for each degree of increase in range. For the present this ratio may be assumed to remain constant.

When the solstices are at right angles to the apsides the amount of light and heat received from the sun by either hemisphere during winter or summer is exactly equal to that received by the opposite hemisphere during its corresponding seasons. The amount so received may be denominated the *normal accession.* When, however, the solstices coincide with the apsides that hemisphere whose winters occur in aphelion while its summers occur in perihelion receives a less than normal amount of light and heat in winter, and a greater than normal amount in summer, owing to the variation in the earth's distance from the sun at these seasons. *If, then, terrestrial temperature is a function of solar accession*, the annual thermometrical range on the hemisphere so situated must be greater than the normal; while at the same time the thermometrical range must be diminished on the opposite hemisphere. Manifestly, too, any increase in the eccentricity of the terrestrial orbit must intensify this effect, since solar accession varies as the square of the solar distance.

In table II the solar accession in winter and summer when the solstices and apsides coincide and the eccentricity of the terrestrial orbit is as at present (0·0168), is compared with the normal, values being expressed in degrees Fahrenheit. Table III exhibits like values for an eccentricity of 0·0747, such as occurred 850,000 years ago according to Croll's calculation from LeVerrier's formulæ.* Both tables are graphically depicted in the accompanying diagram.

These tables were computed as follows:—The relative solar

* "Climate and Time," p. 319. Stockwell computes the maximum eccentricity to be 0·0693888 ("On the Secular Variations of the Elements of the Orbits of the Eight Principal Planets," Smithsonian Contributions to Knowledge, No. 232 (1872), p. xi); but his memoir was not accessible when the table was prepared. The slight diminution in normal accession accompanying increased eccentricity is also neglected.

TABLE II.

Variation in summer and winter accession of solar heat when solstices coincide with apsides, with eccentricity of 0·0168. Aphelion intensity, ·967; perihelion intensity, 1·034.

Latitude.	Winter.						Summer.						Increase in Annual Thermometrical Range.	
	Normal Accession.	Aphelion.		Perihelion.		Difference between Aphelion & Perihelion Accession.	Normal Accession.	Aphelion.		Perihelion.		Difference between Aphelion & Perihelion Accession.	Above Normal.	Above Opposite Hemis.
		Accession.	Below Normal.	Accession.	Above Normal.			Accession.	Below Normal.	Accession.	Above Normal.			
0°	+114·8°	+103·2°	11·6°	+126·8°	12·0°	23·6°	+98·8°	+87·7°	11·1°	+110·3°	11·5°	22·6°	23·1°	46·2°
10	85·4	74·7	10·7	96·4	11·0	21·7	119·2	107·4	11·8	131·2	12·0	23·8	22·7	45·2
20	47·1	37·8	9·3	56·8	9·7	19·0	129·9	117·7	12·2	142·4	12·5	24·7	21·8	43·7
30	1·3	−6 7	8·0	9·5	8·2	16·2	129·9	117·7	12·2	142·4	12·5	24·7	20·4	40·9
40	−49·9	56·1	6·2	−43·5	6·4	12·6	120·6	108·7	11·9	132·6	12·0	23·9	18·3	36·5
50	105·9	110·4	4·5	101·4	4·5	9·0	103·6	92·3	11·3	115·1	11·5	22·8	15·9	31·8
60	161·1	163·8	2·7	158·5	2·6	5·3	79·2	68·7	10·5	89·9	10·7	21·2	13·3	26·5
70	206·1	207·4	1·3	205·0	1·1	2·4	57·8	48·0	9·8	68·0	10·2	20·0	11·2	22·4
80	230·1	230·5	0·4	229·8	0·3	0·7	50·7	41·1	9·6	60·5	9·8	19·4	10·1	20·1
90	239·0	239·0	0·	239·0	0·	0·	49·8	40·2	9·6	59·6	9·8	19·4	9·7	19·4
Mean.	+36·1	+27·1	9·0	+45·5	9·4	18·4	+113·3	+101·9	11·4	+125·5	12·2	23·6	21·0	42·0

TABLE III.

Variation in summer and winter accession of solar heat when solstices coincide with apsides, with eccentricity of 0·747. Aphelion intensity, ·856; perihelion intensity, 1·115.

LATITUDE.	WINTER.						SUMMER.						INCREASE IN ANNUAL THERMOMETRICAL RANGE.	
	Normal Accession.	APHELION.		PERIHELION.		Difference between Aphelion & Perihelion Accession.	Normal Accession.	APHELION.		PERIHELION.		Difference between Aphelion & Perihelion Accession.	Above Normal.	Above Opposite Hemis.
		Accession.	Below Normal.	Accession.	Above Normal.			Accession.	Below Normal.	Accession.	Above Normal.			
0°	+114·8°	+64·0°	50·8°	+169·6°	54·8°	105·6°	+98·8°	+50·1°	48·7°	+151·3°	52·5°	101·2°	103·4°	206·8°
10	85·4	38·7	46·7	135·7	50·3	97·0	119·2	67·6	51·6	174·8	55·6	107·2	102·1	204·2
20	47·1	6·2	40·9	91·5	44·4	85·3	129·9	76·8	53·1	186·9	57·0	110·1	97·7	195·4
30	1·3	−34·4	35·7	38·5	37·2	72·9	129·9	76·8	53·1	186·9	57·0	110·1	91·5	183·0
40	−49·9	77·0	27·1	−20·6	29·3	56·4	120·6	68·8	51·8	176·2	55·6	107·4	81·9	163·8
50	105·9	125·0	19·1	85·3	20·6	39·7	103·6	54·3	49·3	156·6	53·0	102·3	71·0	142·0
60	161·1	172·3	11·2	149·1	12·0	23·2	79·2	33·4	45·8	128·6	49·4	95·2	59·2	118·4
70	206·1	211·0	4·9	201·0	5·1	10·0	57·8	15·0	42·8	103·6	45·8	88·6	49·3	98·6
80	230·1	231·4	1·3	228·7	1·4	2·7	50·7	9·0	41·7	95·6	44·9	86·6	44·6	89·3
90	239·0	239·0	0·	239·0	0·	0·	49·8	8·2	41·6	94·7	44·9	86·5	43·2	86·5
Mean.	+36·1	−3·5	39·6	+78·8	42·7	82·3	+113·3	+63·1	50·2	+168·2	54·9	105·1	93·7	187·4

intensity at the various terrestrial latitudes has been calculated by Meech* and expressed in arbitrary units, each representing $\frac{1}{80}$ of the intensity under the equator at the time of the vernal equinox. The mean for the whole earth, in the same units, is 66·73. Dove had previously, as a result of an elaborate series of observations, determined the actual mean temperature of the earth to be about 58° F. According to Herschel's determination of the temperature of space (which agrees pretty closely with that of Pouillet), or −239°, this temperature is 297° higher than that of stellar space. Each of Meech's units is, therefore, so far as the whole earth is concerned, equal to 4·45° F. This coefficient has also been assumed to be constant; and the intensities, both normal and corresponding to the different degrees of eccentricity, have been reduced to degrees Fahrenheit by its use. The means were computed by the use of the following coefficients:

Latitude.	Coefficient.
0°	1·000
10	·826
20	·658
30	·500
40	·357
50	·234
60	·134
70	·060
80	·015
90	·001

The increase in thermometrical range beyond normal in tables II and III is 21·0° and 93·7° respectively. Making use of the coefficient already determined (0·23°), it appears that these values are equivalent to a diminution in mean temperature over the hemisphere whose winters occur in aphelion of 4·83° and 21·55° respectively, and to a like increase in the temperature of the opposite hemisphere.

It may be added that aside from the specific relations pointed out, the alternate free summer precipitation and rapid winter congelation of seasons varying so widely in temperature would certainly facilitate the formation and conservation of glacier ice.

In the foregoing pages two variable factors have been assumed to be constant. These are the ratios (1) between increase in thermometrical range and diminution of mean temperature, and (2) between solar accession and terrestrial temperature. The first of these ratios appears to augment rapidly with diminution of temperature, as may be seen from a glance at

* "On the Relative Intensity of the Sun's Light and Heat," vol. ix of Smithsonian Contributions to Knowledge, 1856.

table I—indeed comparison of a larger number of observations yielded a larger coefficient. Accordingly, since the temperatures dealt with in tables II and III are collectively lower than those collocated in table I, the use of the coefficient adopted is probably perfectly safe. With respect to the second ratio, the lack of correspondence between observed and computed temperatures indicates that results obtained by its use are excessive. Comparison between observed and computed temperatures will, however, afford the means of eliminating errors arising from this cause. Thus, the actual diminution of terrestrial temperature from equator to pole is about 80° according to Dove, while it would be about 212° if proportional to the solar accession as computed by Meech. Reducing the figures 4·83° and 21·55°, derived from tables II and III respectively, in the ratio of 212 : 80, yields 1·82° and 8·13° as tolerably trustworthy values for the diminution of mean temperature effected by the operation of the law stated at the outset.

Applying the first of these values to the earth in its present status, it would appear that the temperature of the southern hemisphere ought to be about 3·5° lower than that of the northern. The approximate coincidence between this result and those derived from observation strengthens the conviction that the principles detailed in the foregoing pages must be valid. Applying then the second value to the earth when the eccentricity is near its superior limit, it appears that the hemispheres should vary in mean temperature by no less than 16°—that secular summer should prevail in one, while the other was enshrouded in the snows of its secular winter. The importance of the agencies described will perhaps be more manifest when it is borne in mind that during its secular summer more solar heat and light is received by a hemisphere in winter than in summer, while on the opposite hemisphere the solar accession is no less than $1\frac{11}{15}$ times greater in summer than in winter.

Art. LVIII.—*The Stereoscope, and Vision by Optic Divergence;* by W. LeConte Stevens.

[Continued from page 362.]

In a previous article* it has been shown that Brewster's theory of binocular perspective is insufficient to explain vision through the stereoscope when the visual axes diverge. It takes account of only one of several elements which combine to determine the judgment of distance, and the significance of this

* This Journal, Nov., 1881.

should be referred to the sensation of muscular strain rather than to the intersection of visual lines.

The effect of varying the tension of the rectus muscles of the eyes in modifying the estimate of relative distance has been applied in Wheatstone's pseudoscope* and in his reflecting stereoscope, though no reference in this connection has been distinctly made to anything beyond variation of convergence. The following experiment is not difficult. Upon a large sheet of paper a series of vertical parallel lines are drawn, 50^{mm} apart; the last line of this series forms the first of a second series 60^{mm} apart, and in like manner this introduces a third series 70^{mm} apart. Gazing at the first series, as if regarding a remote object, the paper being 1^{m} distant, the images of the lines are soon combined by diminished convergence. Passing slowly to the second series, the convergence is still farther diminished, and it passes into divergence when the third is successfully combined. The apparent distance of the first series I estimate at $2^{m}{\cdot}5$, of the second about 3^{m} and of the third about $3^{m}{\cdot}5$. By intersection of axes, the first should be 6^{m}, the second infinity, and the third -6^{m}, my interocular distance being 60^{mm}. The experiment may be varied in many ways; different observers form different estimates of distance, but I have found none who succeeded in attaining divergence thus without observing an apparent recession of the external image.

To ascertain whether divergence of axes is unconsciously practiced in the use of the stereoscope, I examined 166 stereographs taken at random and found the foreground interval to vary between 60^{mm} and 95^{mm}, the mean being $72^{mm}{\cdot}9$. The average interocular distance for adults is a little less than 64^{mm}; to combine without the stereoscope, therefore, divergence is nearly always necessary. To ascertain the mean deviating power of the lenticular prisms used in the best instruments, 30 pairs were obtained through the courtesy of Mr. H. T. Anthony, of New York. With but slight variation, the focal length was found to be $18^{cm}{\cdot}3$. Mounting each pair in succession, parallel rays 64^{mm} apart were transmitted and received upon a screen $18^{cm}{\cdot}3$ distant. The mean interval between points of light caught on the screen was $79{\cdot}1^{mm}$; hence if rays be sent from corresponding stereograph points, separated by a wider interval than this, they will be not quite parallel after emergence from the prisms, and the eyes must diverge to receive them; 80^{mm} may be taken as a limit beyond which most persons will find divergence necessary if binocular combination in the stereoscope is successfully attained. As this limit is not unfrequently exceeded, axial divergence, unconsciously attained, is quite common, though

* Phil. Transactions, 1852, part I; or. Phil. Magazine, 1852, pp. 506–523.

in extent it rarely exceeds 2° or 3°. I have attained 7°, and Helmholtz * gives 8° as his limit. Several persons of my own acquaintance have been found able to secure divergence without the stereoscope, and their estimates of the apparent distance, size and motion of the external image under various conditions have not differed much from my own.

In the discussion of normal binocular vision, the expression "point of sight" may be applied theoretically to the intersection of optic axes. Its *apparent* position, though not mathematically determined, may be estimated with more or less error, according to the skill of the observer. But in discussing the stereoscope such a definition has to be totally abandoned. The point of sight then is only the point in space to which the observer mentally refers the binocular combination of images formed on corresponding retinal points, where the visual axes, whether convergent, parallel or divergent, meet the retinas. Its apparent position has to be estimated, not determined by intersection of lines. In this estimation the relation between the visual axes is only one of a number of elements that are combined in the formation of a judgment, whether vision be normal or abnormal. Even if stereographs are selected from which physical perspective is in great measure eliminated, the optic angle may be negative; and, when positive, its effect is still antagonized by the disturbance of coördination between focal and axial adjustments, or by the observer's unconscious recognition of the circumstances under which he has been accustomed to view an object of the kind represented. A mountain will never be judged to be so near as a mere diagram, even though the axial relations be similar in viewing the pictures separately. In the stereoscope before me I place a pair of conjugate diagrams representing a skeleton cone, alternately approximating and separating them, in a transverse vertical plane, so that the optic angle varies between +8° and −3° 45′. The apparent distance varies between 30^{cm} and 40^{cm}; if determined by the optic angle it should vary between $+43^{cm}$ and -92^{cm}. The distance of the card remains constant, and tends to keep the focal adjustment so, while the eyeballs are rotating outward, tending to produce adaptation of focal adjustment to a greater distance, the two adjustments being usually consensual. We are in the habit of associating diminution of convergence with increase of distance of the object of sight. As long as the eyeballs continue rotating outward, therefore, the object appears to recede and to enlarge correspondingly, the recession being fastest during the change from marked convergence to parallelism. It does not seem possible to express this apparent rate in mathematical terms.

* Optique Physiologique, p. 616.

The experiment just described does not imply any unusual conditions in the stereoscope except that the higher value, 8°, given to the optic angle is greater than usual. Assuming $72 \cdot 9^{mm}$, the mean already found for the stereographic foreground interval, the corresponding angle of convergence after allowing for deviation of rays is a little less than 2°: the intersection of axes is hence still far from the point to which the focal adjustment is adapted. This fact explains the difficulty experienced by so many persons in obtaining distinct vision through the stereoscope, especially those who have passed beyond middle age and lost in great measure the power of focal accommodation.

Most of the stereographs in common use are pictures in which physical perspective is strong. When these are properly mounted and viewed in the stereoscope the chief advantage gained by use of this instrument seems to be that it necessitates variation in the relation between the optic axes, in order that perfect binocular combination of the different parts of the superposed retinal images be secured in the subjective Cyclopean,* or combined binocular, eye. If there is perfect superposition of retinal points on which the foreground of the stereograph is imaged, there is necessarily imperfect superposition of those on which the background is imaged. If the attention is then given to the background, slight outward rotation of the eyeballs is necessitated, and this is habitually associated with recession of the point of sight. Whether with axial divergence binocular relief is instantly perceptible, as in Dove's experiments with axial convergence, by illumination of the stereograph with the electric spark, I am unable yet to say; I hope to test this at no distant day. It should be so according to Professor LeConte's theory of binocular perspective.†

What has been generally given and accepted as the mathematical theory of the stereoscope applies strictly, but only to the relations involved in taking the photographs with cameras appropriately placed, so that the axes of the lenses converge upon some point of the object pictured. When the negatives are once fixed and proofs from them so mounted that corresponding points from the pair are focalized upon corresponding retinal points for the observer who binocularly combines them, with or without the stereoscope, the relation between the different parts of the fields of view combined undergoes no sensible variation, real or apparent, except between the limits fixed by difference between the stereographic intervals in the background and foreground respectively. If the eyes are comfortable, after binocular combination is attained, it makes little

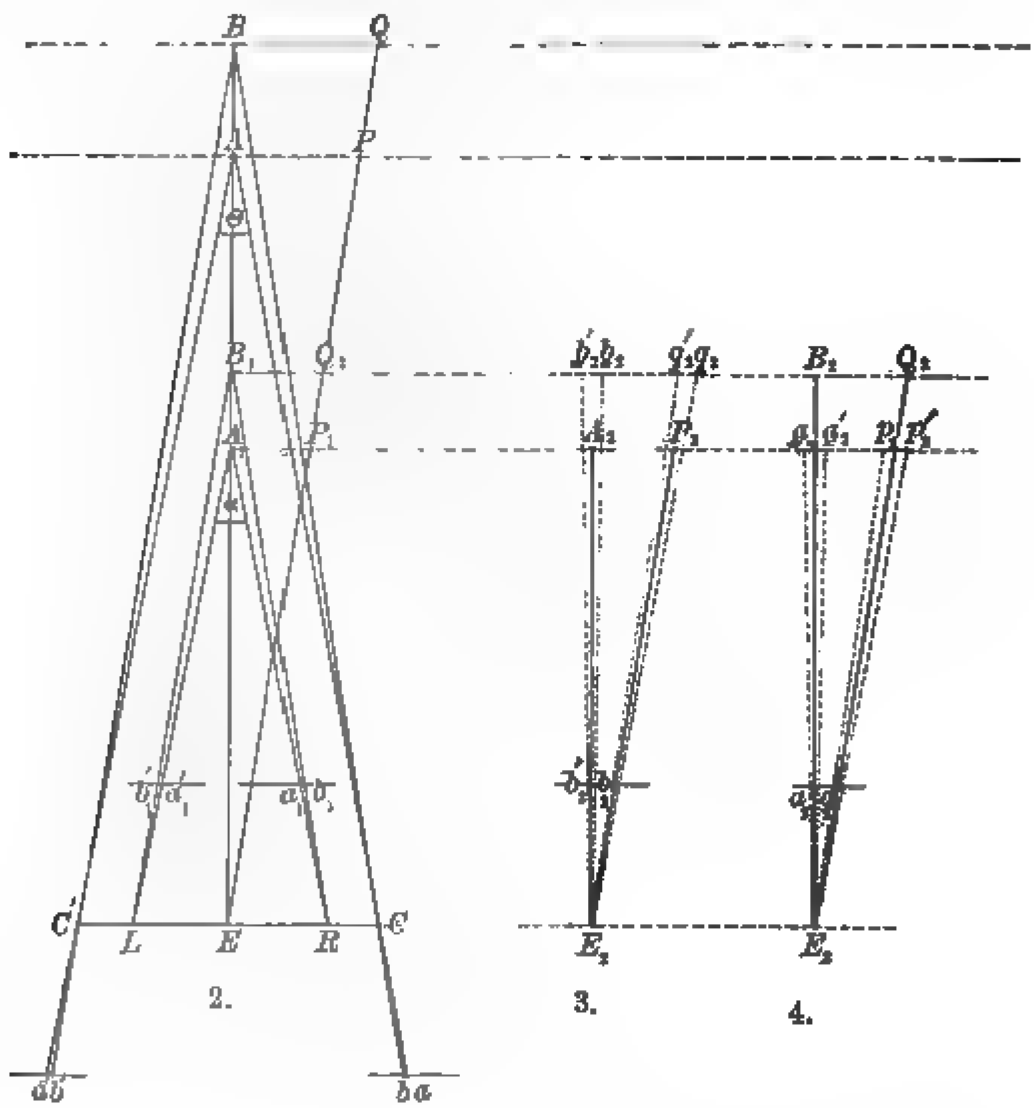
* This Journal, III, vol. i, p. 33 et seq.
† This Journal, III, vol. ii, p. 3.

difference whether, at a given moment, the visual axes are convergent, parallel, or divergent. The combined external image as a whole is made to appear nearer by convergence and farther by divergence, but this has no perceptible effect upon the ratio between the distances of its different parts. Though the distinctness in separation between foreground and background is greatly enhanced by the slight variation in axial relations that is necessitated, the estimation of absolute distance is determined mainly by physical perspective, and by comparison of the picture with known realities to which it bears some easily recognizable relation. In the few cases where reversion of perspective is plainly produced by transposing the pictures composing the stereograph, it will be found that the difference between background and foreground intervals is large, and that some of the elements of physical perspective are relatively weak. I have examined a number of such transposed stereographs and found the effect in many cases to be not distinct reversion but rather confusion. Sometimes in one part of the picture reversion is noticeable, while in the rest there is only decrease in the apparent distance between background and foreground. The conflict between physical and physiological perspective results in a judgment not wholly in obedience to either; generally the former prevails, but the weakness of the residual effect is perceived by contrasting it with that obtained by squinting and thus reversing the sense of the physiological element. The judgment may be regarded as a compromise rather than an independent selection. In vision by divergence, and in vision through the stereoscope generally, the *binocular relief* is largely due to the *variable relation between the optic axes*, as different parts of the stereograph are examined; while the judgment of absolute distance is mainly due to *physical perspective and comparison with remembered realities*; it is modified by focal adjustment, and is in practice nearly, but not quite, *independent of the optic angle.* This remark would not apply if the optic angle were very large.

No diagrams can ever represent with perfect accuracy the apparent positions of objects seen in the stereoscope. If we neglect such disturbing influences as arise from conflict between focal and axial adjustments, and from difference between the optic angle and that between the camera axes when the pictures were taken, and also disregard the fact that the surface of the retina is curved while that of a photograph plate is plane, the following method perhaps will give the best results.

In fig. 2, let C and C′ be the centers of two camera lenses whose principal axes are as usual parallel, and a pair of secondary axes forming an angle, θ, in a horizontal plane, are di-

rected upon an object, A, in the foreground of a scene. Let E be the midpoint between C and C′; then EA is a median; on this prolonged let B be an object in the background. Parallel to C′ C and to the vertical plane of the sensitized plates

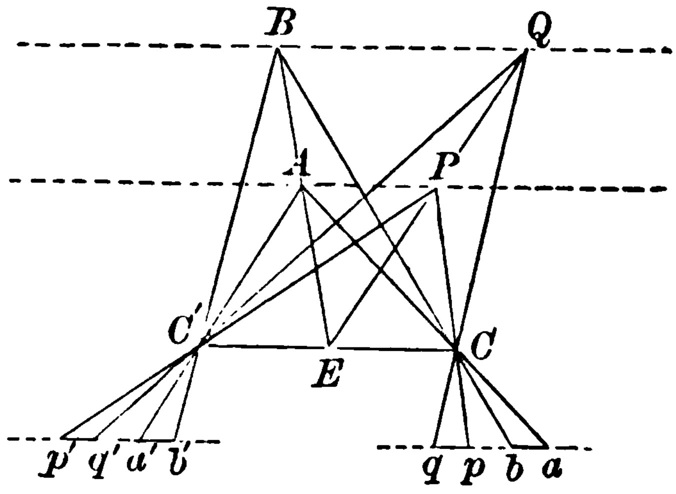


behind the lenses, let two planes be passed through A and B respectively. Let P and Q be any points on these planes, so related that the straight line Q P passes through E. On the plates the stereoscopic displacements of the projections of B from those of A are $a\,b$ and $a'\,b'$; and it may be easily shown geometrically that the displacements of those of Q from P are each equal to $a\,b$. This is not shown in the drawing, but a glance at fig. 5 is sufficient.

Let E be midpoint also between a pair of eyes, R and L, in front of which the conjugate photographs are placed after being inverted, and let rays from them be so deviated by semi-lenses as to make $\alpha=\theta$. If the ratio of L R to C′ C be known, the distances E A_{1}, E B_{1}, E P_{1} and E Q_{1} are determined. In binocular vision the direction of the object seen is always estimated from the position of the combined binocular eye, E, and is coincident with that of the median between the two visual

axes, but always somewhere in front.* This is universally true for normal eyes, as may be abundantly learned by experiment, whether the axes be convergent, parallel or divergent, and whether the median be at right angles or oblique to the interocular line, LR. In fig. 4, if E_2A_2 and E_2P_2 represent these medians, we have both direction and distance determined for these foreground points. To the right eye, B_1 (fig. 3) appears beyond and to the right of A_1 at an angular distance determined by the stereoscopic displacement, a_1b_1; to the left eye, beyond and to the left at an equal angular distance; to the binocular eye, E_2 (fig. 4), it is hence homonymously doubled at b'_2b_2. To secure single vision of it, the optic angle must be diminished, and through the rectus muscles this at once suggests to the mind increase of distance, producing at the same moment heteronymous doubling of the foreground point A_2, as in fig. 4. Similar remarks apply to P_2 and Q_2.

5.



If α be less than θ, as is often the case, this fact will cause the observer to estimate A_2 to be more distant than it is represented in the drawing, but by no means necessarily so distant as the actual vertex of α. If α be reduced to zero or become negative the sensation of still further change of muscular tension makes the apparent position of A_2 recede still more, and also that of B_2 in the same proportion; but in no case is this determined by intersection of visual axes except when $\alpha=\theta$. No one can have failed to notice the exaggeration of perspective in some stereoscope pictures, produced by making θ large while α is rendered small or negative by mounting the pair too far apart. This indeed was noticed by Wheatstone,† who approaches very near to the idea of possible optic divergence accompanying the perception of binocular relief, when he says, "but I find that an excellent effect is produced when the axes are nearly parallel by pictures taken at an inclination of 7° or 8°, and even a difference of 16° or 17° has no decidedly bad effect." His preconception that optic convergence, even though slight, is indispensable, prevented his apprehension of more than part of the truth. He states, as a remarkable peculiarity, that "although the optic axes are parallel, or nearly so, the image does not appear to be referred to the distance we should, from this circumstance, suppose it to be, but it is perceived to be much nearer." Such large angles as 17° are sel-

* This Journal, III, vol. i, p. 33 et seq.

† Wheatstone, Physiology of Vision, Phil. Mag., 1852, pp. 513–514.

dom resorted to at present. For taking stereographs of statuary, etc., the lenses of the binocular camera are not often more than 80^{cm} or 100^{cm} apart.

That muscular tension is more important than mere intersection of axes in affecting the judgment of distance and size may be shown by aid of Wheatstone's reflecting stereoscope. Having placed the two outline drawings, each 20^{cm} from its mirror so that a distinct combination is attained by axial parallelism, the judgment of distance is as definite as could be desired. Upon converging the axes strongly and giving attention successively to the two monocular images thus obtained, each appears greatly diminished in comparison with the binocular image just seen. Moreover, at the moment one of them is made an object of special attention, the other grows slightly larger. We have thus images of three apparent sizes, according to the degree of muscular tension with which they are separately regarded, while the visual angle remains constant. The visual axes are converged until their intersection is not more than 5^{cm} or 6^{cm} off, and the illusive impression is that each image is in the direction of its own axis much beyond the intersection. But in fact, being monocular images, the direction of the center of each is that of a secondary axis, the right eye perceiving that on the right, instead of the left. Since the optic center and center of rotation are about $6{\cdot}6^{mm}$ apart, the former being displaced toward the nasal side during the experiment, the two secondary axes meet at a very distant point in the rear. While the distance of the monocular image is indeterminate, it is judged easily enough to be *not* at the vertex of either the apparent or real angle determined by the meeting of axes. The experiment is very striking and is not difficult. We have a binocular image, of little more than natural size, with clear judgment of distance, as the result of axial parallelism; two monocular images, of diminished and separately variable size, with very uncertain judgment of distance, as the result of axial convergence, the principal and secondary axes being subjectively interchanged. The apparent diminution in size of the monocular images may be easily observed by crossing the eyes, while holding in front a card on which a sharply defined outline is drawn. I may discuss this still further in a future paper.

No theory of the stereoscope that includes axial divergence is possible, unless we recognize the subjective combination of the two eyes into a single central binocular eye as the point of origin in all perceptions of direction, distance and form. What is essential for binocular vision is not any particular relation between visual axes but rather superposition of retinal images in the binocular eye. What seemed uppermost in the minds of Wheatstone and Brewster * was superposition of external vir-

* Wheatstone, Physiology of Vision, Phil. Mag., 1852, pp. 243 and 246. Brewster, on New Stereoscopes, Phil. Mag., 1852, pp. 17–26.

tual images by causing rays from two pictures to deviate and appear to come from one central combined external picture. This would exclude the possibility of optic divergence, but seems to be still the most generally accepted theory of the stereoscope. In securing dissimilar pictures of the same object by convergence of camera axes we secure the conditions for the perception of binocular relief by divergence of visual axes.

In the diagram attention is called to the identity in position between the optic center of the binocular eye and the only point through which lines can be drawn in such a way as to cause the stereoscopic displacement to be constant for projections of the points where these lines cut the foreground and background planes. This fact alone is enough to suggest that in vision through the stereoscope the midpoint between the eyes must be the point of origin from which distance and direction are to be perceived. A truth that was first recognized as a physiological necessity is thus confirmed by purely mathematical considerations.

The dissociation between focal and axial adjustments in forced convergence or divergence is at first troublesome and productive of indistinct vision, but this vanishes in great measure after a little practice in ocular gymnastics. If the eyes are comfortable we are apt to forget that the vision is abnormal, and to assume that conditions exist which belong only to normal vision. To ascertain what modifications are imposed by physiological conditions upon the generally accepted mathematical theory of the stereoscope has been the chief object of the present investigation.

New York, Sept. 16, 1881.

Art. LIX.—*On the relation of the so-called "Kames" of the Connecticut River Valley to the Terrace-formation*; by James D. Dana.

Since the publication of my papers of 1875 and 1876 on the stratified drift of Southern New England treating especially of the character and effects of the flood closing the era of ice, large additions have been made to our knowledge of the terraces of the Connecticut Valley, and of some other parts of Northern New England, through the New Hampshire Report of Mr. Warren Upham, published in 1878.* In his Report, Mr. Upham describes in detail the stratified drift or terrace-

* Geology of New Hampshire, Part III, Chapter i, Modified Drift in New Hampshire, by Warren Upham, pp. 3–177. 1878. A synopsis of Mr. Upham's Report, by its author, was published in this Journal, vol. xiv, p. 459, 1877.

formation of the valley; gives the heights of the terraces above the river (and above mean tide) from careful levelings along its course, commencing near the source of the river in Connecticut Lake, 1618 feet above the sea; discusses the origin of the deposits and of their various features; and presents his very valuable topographical details on a map of the valley occupying a series of plates. Besides the ordinary stratified drift, Mr. Upham finds gravel ridges or deposits to which he applies the name "Kames." According to his observations, the "kames" were formed before the deposition of the beds of the terrace-formation and after that of the till or unstratified drift, so that they represent an intermediate stage in the progress of the era and call for special explanations.

The facts from the Merrimac Valley also are presented in a similar way, and with like deductions.

In the study which I had made of the Quaternary of Southern New England, and less perfectly of drift-phenomena elsewhere, I had been led to refer all the stratified drift above the till to the terrace-formation; and no later observations in river valleys had resulted in the discovery of any thing answering to Mr. Upham's "kames." During the past summer, I have been over the region of the Connecticut Valley described by Mr. Upham, in order to obtain a full understanding of his facts, so as to be able to incorporate them with the knowledge I had previously acquired, and I here give an account of what I observed, with my conclusions.

That the subject may be rightly apprehended, I preface my statement with a brief mention, first, of some of the general facts respecting the stratified drift-deposits which I had gathered from personal study, and, next, of the facts and deductions which are brought out by Mr. Upham with relation to the "kames."

I.—(1.) Scratched bowlders and till are almost uniformly absent from the valley terraces of New England and from the stratified beds that make the terrace-deposits. Exceptions occur where the underlying rocks having till over them come so nearly to the surface of any terrace that the till outcrops.

(2.) The layer of till of the hill-slopes is continued beneath the terrace deposits; showing that along the valleys the till with the bowlders was generally deposited first.*

* In the street adjoining my own house, in New Haven, a trench, excavated for a sewer, passed through ten feet of stratified drift, or of the terrace formation, and then opened into a deposit of gravel and scratched stones (including some bowlders of eight to ten cubic feet); and, below two or three feet of this kind of material, entered the Mesozoic sandstone of the region. This sandstone rises in a ridge, above the level of the terrace, 400 yards to the north of the excavation, and must have constituted both the shore and bottom of the valley-waters at the time of the deposition.

(3.) The stratified drift of the valley consists ordinarily of fine material below, and coarser toward or at the top; the bottom portion being commonly of clay or loam, or fine sand with frequently more or less clay; then, following this, layers of sand often fine, but often with more or less gravel; then above, toward the top in the upper fifteen or twenty feet, coarser gravel, and in some regions cobble-stone beds; an order of arrangement, which indicates—in accordance with ordinary hydraulic principles—that the flow of the depositing waters was, as a general thing, less rapid at the time of the early depositions, and most so during the later or that of maximum flood. Exceptions exist along those streams that were torrents, and sometimes at the mouths of tributaries to large streams.

An uppermost sandy layer, of two or three feet thickness, frequently exists, indicating that the ebb commenced in a lessened rate of flow.

(4.) The portion of the terrace formation in a river valley that is nearest to the river or adjoins the channel-way, may, and often does, consist largely of beds of coarse gravel or cobble-stones, while one or two hundred yards away from the river it is composed chiefly or wholly of beds of sand; the river-border deposits being thus coarse because of the sifting or assorting action of the stream in violent flow along its channel or against one or the other side of it. And the coarseness may diminish down stream, because of greater remoteness from the source of coarse material, and also because of a change in the rate of flow, producing less power of transportation and so allowing of a deposition of the sands drifted out above.

(5.) Terraces of different degrees of coarseness and of different heights were sometimes simultaneously made on opposite sides of a stream, owing to the different rates of flow in the waters along the two sides.*

* Along the middle one of three streams entering the New Haven Bay, called Mill River, coarse gravel and cobble-stone deposits characterize the New Haven terrace-formation all the way to the harbor; they are vastly coarser on the west side of the stream than on the eastern, and in the southern part of its course are most so along a more western line away from the present stream. Moreover, the deposits make a terrace on the *west* side of the stream of only twenty-five feet above mean-tide level, while on the *east* side, where the material is so much less coarse, they rise to a height of forty-three to forty-five feet, or the ordinary level for the New Haven plain at that distance from the Sound. Those coarsest beds were made under the sifting action of the violently flowing waters (the pitch of the stream for some miles back being eight to ten feet a mile), and hence, that is, because of the loss of the finer material in this way, the height attained on the side of most rapid flow was twenty feet below the normal height. Moreover, the violent waters were probably those of the nearing maximum stage of the flood; for the coarse gravel deposits (as various sections show) extend down but fifteen feet from the surface, and rest on beds of sand and fine gravel.

(6.) The terrace-formation of a large and broad valley was made mainly, not from what its river transported, but from the contributions of tributaries. Consequently, (*a*) the height of the maximum flood is best registered in terraces at the mouths of tributaries, and (*b*) where tributaries fail for long distances, there may be only low terraces; further, (*c*) the coarsest gravel beds should exist in the deposits about the mouths of tributaries, and especially in those made along the banks of the *main river* near these mouths, where the contributions were subjected to the sifting action of the swiftly flowing river.

(7.) The extent and height of the terraces made along any part of a valley depended not merely on the amount of contributed material, but also largely on the size and form of the valley. Where very wide and deep, like many lake basins, the deposits were generally sufficient to make only low or narrow terraces; where narrow, the flow of waters was sometimes, because of the diminished width, too rapid for any depositions; but where the valley, though narrow along the main channel had a broad region of ledges on either side that became overflowed when the waters were nearing their maximum depth, a high terrace might then have become of great width; for the shallow region favored deposition by offering resistance to the flow, and however wide needed little material to cover it. Just as this condition favored the making of a broad upper terrace, so it favored the making of a wide terrace at lower levels, especially if the flow of water continued long at those levels.

(8.) Ice-floes, bearing sand, gravel and bowlders, added to the transported material for the terrace-formation; and they should have been abundant during the breaking up, at the time of maximum flood. Being carried by the waters, their distribution of material would have taken place for the most part in accordance with the principles above explained.

II. Mr. Upham adopts in his New Hampshire Report, the view that the valley formations are deposits made by the flood from the melting glacier, and it appears from his explanations that he would accept without objection several of the above explanations. The points of discrepancy, however, are many and important. I cite here only those relating to the "*kames*," and mostly in the author's words. The term *modified* drift is used by him for *stratified* drift.*

Page 12. "The oldest of the deposits of modified drift are long ridges, or intermixed short ridges and mounds, composed of very coarse water-worn gravel or of alternate layers of gravel and sand irregularly bedded." "Their position is generally along the *middle* or *lowest parts* of the valleys." Wherever the

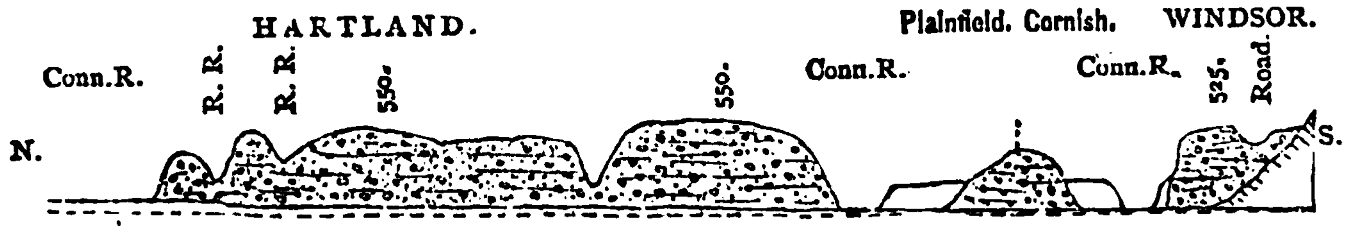
* I have avoided the term *modified*, because it is not known to express in all cases the truth, preferring the non-committal term *stratified*.

ordinary fine alluvium of any terrace occurs adjoining a kame, "it overlies or in part covers the kame deposits," the ordinary terraces being of later formation than the kames.

Page 43. Along the Connecticut, between Vermont and New Hampshire "from Lyme to Windsor, a continuous gravel ridge or kame extends 24 miles, along the middle and lowest portion of this valley, with its top 100 to 250 feet above the river." "Its material is gravel and sand in irregular obliquely-bedded layers, always showing an inclined, and in most cases a distinctly anticlinal or arched stratification. The gravel, which always forms the principal part of the ridge, varies in coarseness from layers with pebbles only 1 or 2 inches in diameter to portions where the largest measure 1½ or 2 feet. The fine kinds prevail." "The sand is usually coarse and sharp, well suited for masons' use; it occurs in layers of varying thickness up to one or two feet, but sometimes it is wholly wanting." "All the materials of this kame, and of its remnants along this valley, are plainly water-worn and stratified."

Page 44. "The most important feature of this kame, if we compare it with others in New Hampshire, is that along its entire extent it constitutes a single continuous ridge which runs by a very direct course nearly in the middle of the valley, having no outlying spurs, branches, parallel ridges, or scattered hillocks of the same material associated with it."

1.



Southern part of the "Kame," in Hartland and Windsor.

Page 45. "In calling this kame continuous from Lyme to Windsor, it is not meant to imply that it is now entire, since it has been frequently cut through and considerable portions swept away by the main river and its tributary streams; but that so much of it remains as to make it certain that it originally formed an unbroken ridge." The former southward continuation of the kame below Windsor is stated to be "probable though now shown by only a few fragments." Mr. Upham then mentions, on p. 47, facts from the vicinity of Windsor, showing at one place in the valley "gravel which is unmistakably that of a kame"; just south, what "seems to be a kame deposit;" and 1½ miles south, "distinct remains of the kame," forming the east border of the terrace, both kame and terrace being 150 to 170 feet above the river. For the next 11 miles no indication of the kame are seen; and beyond are only remains at long intervals more or less distinct.

The preceding figure is part of a section, given on p. 45 of the Report, intended to show the general features of the southern part of this kame ridge (exaggerated relatively in height) in Hartland and Windsor: and the following (from p. 40) is a transverse section of the Connecticut valley through the Hart-

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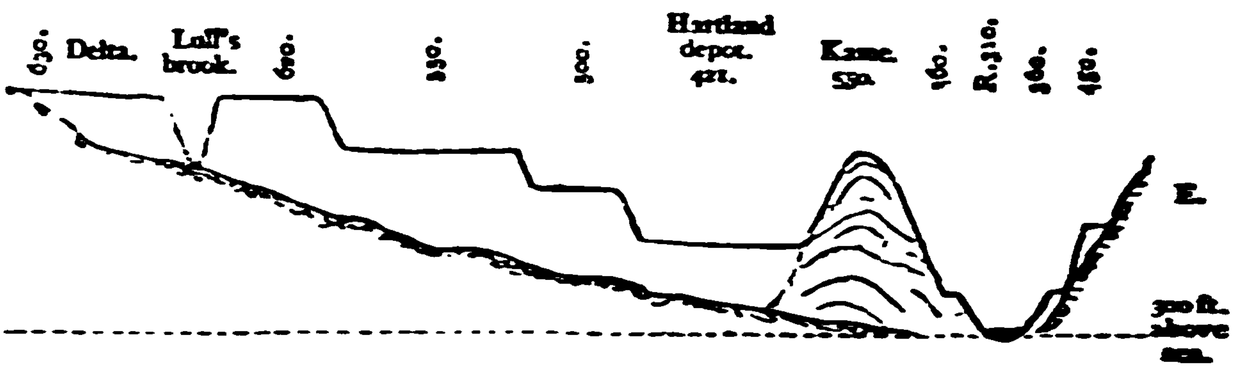
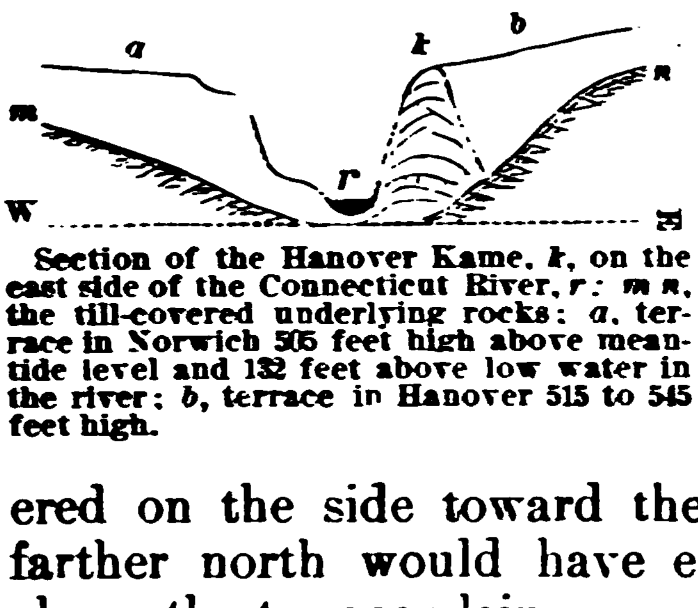


Transverse Section in Hartland and Plainfield.

land deposits, exhibiting the position of the kame just west of the river channel, and its relation to the terrace-formation and the several terraces of the valley.

The adjoining figure, from page 37 of Upham's Report, will help further to explain the author's views. It represents the Hanover "kame," with the outline of the terrace-plains on the opposite sides of the Connecticut. The kame, *k*, is represented as constituting a ridge, coarsely stratified, buried beneath the terrace formation, up to its very top, on the landward side, but uncovered on the side toward the river. A section taken a little farther north would have exhibited the "kame" projecting above the terrace-plain.

3.



Section of the Hanover Kame, *k*, on the east side of the Connecticut River, *r*; *m n*, the till-covered underlying rocks; *a*, terrace in Norwich 505 feet high above mean-tide level and 132 feet above low water in the river; *b*, terrace in Hanover 515 to 545 feet high.

"Kames" are also described as occurring in the valley to the north, but at long intervals.

As to origin:

P. 176. The kames "were deposited, as explained on pages 13 and 14, by glacial rivers, at the final melting of the ice sheet, in *channels formed upon the surface of the ice.* When the bordering ice-walls and its separating ridges and masses disappeared, the gravel and sand remained in long steep ridges, or in irregular short ridges and mounds."

P. 44. The infrequency of angular fragments and bowlders shows "that the kame of the valley was formed in an open ice-channel." P. 14. On the ice in these "channels were deposited materials gathered by the streams from the melting glacier. By the low water of winter, layers of sand would be formed, and by the strong currents of summer, layers of gravel, often

very coarse, which would be very irregularly bedded." "The glacial rivers which we have described appear to have flowed in channels upon the surface of the ice, and the formation of the kames took place at or near their mouths, extending along the valley as fast as the ice-front retreated." P. 44. "When the river entered upon the work of excavating its present channel in the alluvium, the kame was a barrier which confined erosion to the area on one of its sides and protected its opposite side; so that this ridge of gravel often forms the escarpment of a high plain with the river flowing at its base."

The chief points urged by Mr. Upham with regard to the so-called "kames," exclusive of those pertaining to mode of origin, are:—origination: after the till and before the stratified drift of the terraces; material: chiefly beds of gravel; structure: usually arched or anticlinal; situation: generally between the river and the upper terrace, and often making the riverward limit of the latter, also, in many cases, partially isolated and ridge-like, owing to a depression between it and the terrace, and sometimes a large depression; height: frequently the same with that of the upper terrace or a little above it. Further, his descriptions show that he refers coarse cobble-stone deposits in the riverward part of the terraces always to "kames."

In my study of the facts relating to the Connecticut Valley "kames," I commenced at Windsor, the southern limit of the great line of "kames," and examined the valley formations at various places from that place to Lyme, and thence northward to Barnet and Lancaster: and the report I have to make is unfavorable to the "kames." I made levelings at various places in order more surely to identify the terraces mapped by Mr. Upham, and to apprehend their true relation to the Connecticut Valley, and also, to add, if possible to the facts. My trials soon satisfied me as to the essential correctness of his measurements.

Windsor.—At Windsor (on the west side of the Connecticut) the upper terrace of the village rises to a height of about 216 feet above the river or 520 feet above the sea-level. I saw no good opportunity for a satisfactory examination of the material of its lower part beneath the village; but in the upper part found it to be fine sand and loam, though somewhat pebbly through the upper 25 feet.

South of the village lies Ascutney Pond, a north and south body of water made by damming the waters of Ascutney brook; on the *east*, the pond is separated from the Connecticut River by a ridge of stratified material, nearly flat-topped, having about the same height as the upper terrace. Mr. Upham says, somewhat doubtingly, that this ridge "seems to be a kame de-

posit." It ends southward in rocky ledges. A mile and a half farther south, the high river terrace consists along its eastern margin of very coarse gravel, and is pronounced therefore to be in this part the "remains of the kame."

I found this ridge east of Ascutney Pond to consist mainly of loamy material, or sandy loam, like the terrace west of Windsor, with little gravel and that chiefly over its upper surface or in an upper layer. But directly *west* of the Pond there is a terrace (not referred to particularly by Mr. Upham) whose material is made up largely of coarse gravel, in part cobble stones, and coarsest in its upper layers, which in this portion is as much entitled to be called "kame" as that "a mile and a half farther south." This terrace rises westward to a level plain at 448 feet above the sea-level, and then another at 480, and this last rises to 525 feet, which is the height given by Upham for the possible "kame" east of the pond. Its gravelly character continues, but diminishes northward.

I found no evidence whatever that the eastern portion of the terrace was a "kame," that is, a part separate in time of origin from the rest: the evidence was all against such a conclusion. Moreover there was an abundant source at hand for the amount of coarse gravel and cobble stones; for Ascutney or Mill Brook, rising in northwest Reading, flows with rapid descent by the north side of the lofty Ascutney Mountain (3320 feet), and would have been a great transporter from the drift-covered country it drained. The position of the stream, and its relation to the southward-flowing Connecticut, account for the distribution of the "kame" material or coarse gravel of the Windsor region, including that of Windsor village, mentioned by Mr. Upham, and also for the isolation of the ridge on the east side of the pond.

Two miles north of Windsor a kame is entered on Mr. Upham's map. Much coarse gravel here makes the outer or westward portion of the upper terrace, which is by the map 500 feet above the sea-level. Besides coarseness of gravel, I saw no evidence of a kame, that is of any deposits that were distinct from the terrace in original deposition. A brook comes from the west just north of the "kame."

Hartland station, 4½m. north of Windsor.—At this place stands the "kame" ridge represented in Upham's section reproduced, on page 456, of which he says: "At one place, east of Hartland depot, this plain (that of the upper terrace) has been swept away from both sides, and the kame forms a conspicuous steep ridge 125 feet in height [above the depot plain, 240 feet above the river]. Wherever it is exposed, it is readily recognized by the pebbles which strew its surface, and which are very rarely found in the ordinary modified drift of the valley."

I ascended this prominent "kame" with my interest greatly augmented by the description in the Report. The narrow plain between it and the station (see the section) was covered with pebbles from an underlying gravelly layer. The same gravelly layer made apparently the base of the "kame," for some loose cobble-stones were found at the base of the slopes and for 10 to 15 feet above. But on ascending the ridge, no gravel was anywhere observed at a higher level; on the contrary, all was fine loam or fine sandy loam to the very top. And on descending, the same proved to be true; the only gravel was at its base, 50 feet above the river and nearly 200 feet from the top, according to my leveling. There were no good sections, but if made of gravelly layers, loose stones or pebbles would have worked out to the surface and shown themselves somewhere over the earthy sides.

A few rods west and northwest of the Hartland depot there was gravel in the terrace, and much of it; and according to the description of "kames," there was, as far as material goes, a "kame." On the first terrace-plain, about 65 feet (by my leveling) above the railroad track (or 486 to 490 above the sea level) large stones (1 to 10 inches across) lay over the surface, and very many in the sloping section of it facing the railroad track. From this terrace-plain, some rods to the west, there is an abrupt rise to the next higher terrace, and here the material is fine sandy loam with no pebbles. The natural conclusion is that the gravelly stratum is a lower part and the sandy loam an upper part of the same terrace formation, precisely as in the so-called "kame;" and, secondly and accordingly, that the "kame" is nothing but a piece of the terrace-formation. Lull's Brook here comes in from the west and is no doubt accountable for the coarse gravel.

North Hartland, nearly 4 miles north of Hartland.—At North Hartland station, there commences, according to Upham's map, another "kame" a mile long; it is near the river, close by the west side of the railroad. Its height by the map is that of the upper terrace-plain, or 550 to 560 feet above mean-tide level. Very coarse gravel shows itself in an oblique section of the terrace formation or "kame" facing the railroad, becoming cobble-stone layers 70 to 80 feet above the track. The coarseness diminishes to the northward. The large torrential stream, Quechee river, rising in the Green Mountains, enters the Connecticut here, and seemed to be a sufficient source for all the depositions; while the fact that the contributions were contributions to the Connecticut, which was in rapid flow off its mouth, accounted for the distribution of the especially coarse accumulations along the riverward border of the terrace.

In Hartford, Vt., at White River Junction, 4½ miles north of North Hartland.—On the west border of the Connecticut about White River junction, or at the mouth of White River, there is a short "kame" according to Upham's map south of this river, and one, a mile and a half long, north of it. The White River valley is here very broad, like a piece of the Connecticut, and as it rises westward but slowly, it opens to view a portion of the Green Mountain range, which is the chief source of its waters. The Connecticut valley terraces of the region are high—not far from 180 to 235 feet above the river, or 510 to 570 above mean tide level; but that to the north, owing to the retreat in the hills is much the most extensive, and hence the greater length of the northern of the two "kames."

The *southern "kame"* commences within a few yards of the railroad station and hotel, where an excellent section of it is exposed to view. The pitch of the slope toward the Connecticut is about 40°. The structure is well-bedded throughout. The layers consist of cobble-stones, finer gravel and coarse sand. The coarsest cobble-stone layers are below, and some of the rounded stones from them are one to over one and a half feet in diameter. Other cobble-stone layers, less coarse, occur at different levels above, alternating with an increasing thickness of gravel; and toward the top, which is near the top of a ter-

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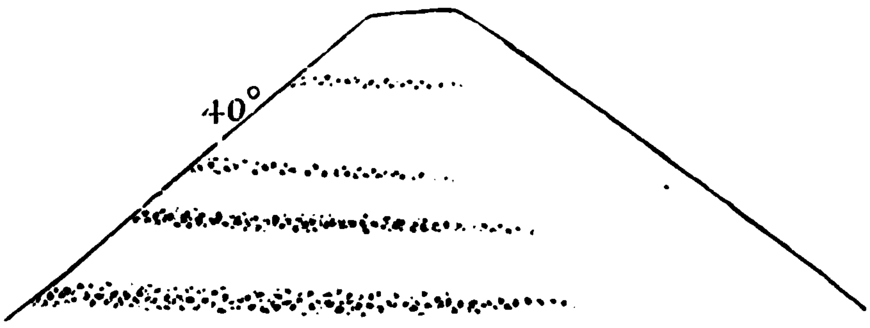

Upper part of the section of the "kame."

race-plain, the material is finer gravel and sand. Fig. 4 shows the position of the cobble-stone beds in the upper half of the section. The beds are not continued through the figure because in the western portion of the section the layers were mostly concealed by slides; but it was manifest from the few and smaller stones on the surface that there was a marked diminution in coarseness to the westward even in the first 100 yards.

The cobble-stone beds exposed to view in the section stop short off below at a level about 20 feet above the level of the railroad track, or 56 feet above the river (low water), and underneath occurs a bed of coarse sand, having the flow-and-plunge structure well marked. A section of the same sand-bed was observed 70 yards to the south, evincing that it is not a local deposit. But the depth to which it was exposed was hardly 8 feet; and it may be that there are other stony layers

underneath. Above the top of this section there is a nearly even terrace-plain, 160–170 feet above the river, or 493 to 503 above mean-tide level. This plain rises to the southwest to a maximum height (not observed by Upham) of 570 feet. The material is fine sand and sandy loam. But along the riverward border of this terrace plain, where it is lowest (493 feet), stands a steep narrow ridge, 50 to 65 feet high, which, judging from the stones of its surface, is made chiefly of beds of cobble-stone gravel. The top is 546 feet (Upham), above the sea.

The cobble-stone character of this ridge and its position make it eminently "kame"-like. But the evidence from the section described, as well as from the plain around, is directly opposed to the idea that it is the top of a buried gravel ridge, existing there before the terrace material was deposited.

In the section, the obvious facts are: that these upper cobble-stone beds—those of the top ridge—are underlaid, first by layers of sand and fine gravel, and then below by alternations of coarser beds; that all the beds are horizontal instead of arched; that they diminish rapidly in coarseness westward, or up White River, showing this even in the first 100 yards, and less rapidly southward or down the Connecticut, the coarsest deposits being at the angle in the terrace formation between the two streams. All the beds are evidently those of the terrace-formation, and the cobble-stone ridge at top is the youngest instead of the oldest.

The *northern "kame,"* or that north of White River, commences about half a mile from the railroad station. A section is exposed to view at its southwest angle, facing White River, exhibiting very similar features to those presented by the northern kame near the railroad. It is horizontally bedded throughout, and the coarsest beds are below; and some of the rounded stones from the beds are two feet in diameter.

But the cobble-stone beds are of less extent, for they reach only to a height of 45 feet above the railroad track, or 81 above the river, and are coarsest at 16 to 26 feet. Above the 45 feet the beds are of coarse and fine gravel, and increasingly finer to the top of the terrace, 510 feet (Upham) above mean tide. Below 15 feet above the railroad the beds are concealed.

On the top of the high terrace, along its riverward border, some spots of cobble-stone gravel occur, but no distinct gravel ridge like that of the southern kame.

The interior of this "kame" is fortunately more or less perfectly exposed to view in both *longitudinal* and *transverse sections*; and it is remarkable that these sections have nothing "kame"-like in them.

The longitudinal or *north and south* section extends along a

cut or gorge commencing close by the west side of the cobble-stone exposure just described. The gorge (with its carriage road at bottom), seemingly divides off a veritable "kame" from the terrace west of it; but the beds on the opposite sides of this cut so correspond, that there can be no doubt of stratigraphic unity.

The section of the "kame" along this gorge is more or less obscured by slides, but not in all parts. It shows, first, that the stony beds diminish rapidly in coarseness away from White river or to the north. One hundred feet up the gorge, the cobble-stones are half smaller and extend up to a height of only 30 feet above the level of the railroad, or 66 feet above the river, and beyond this they continue to diminish. At 400 feet up the gorge, the ascending road along its bottom reaches a height of 28 feet above the railroad level, and here, in the exposed section on the *east side*, there is a bottom layer of sand, and above the sand 30 feet in thickness of clay; and this clay outcrops on the west side of the gorge as well as the "kame"-side, proving that the deposits of the supposed "kame" are one in bedding and material with those of the terrace formation, just as the high terrace plain above the whole (510–520 feet) is one from the Connecticut westward.

To the eastward of this section, toward the railroad, the deposits diminish in coarseness: and the same change continues northward along the railroad, where the surface material of the lower part of the terrace-slope shows stones only to a height of 20 or 30 feet, or less, above the track.

One of the *east-and-west* sections of the "kame" exists about half a mile north of the south end. A gorge intersects the deposit which is cut down to the level of the railroad track and extends inward (westward) to the center of the "kame" line. But there is nothing kame-like within it, and least of all at its inner extremity. On its *north* side, it has no cobble-stone beds, not even gravel beds; the material is fine sand delicately straticulate. On its south side, in the part nearest to the river, there occurs, in a large mass that has slipped down from above, a thin bed of small stones (three inches in diameter) with some gravelly and sandy layers below; elsewhere the material is sand. In the inner part of the cut, besides the fine sand, there is a bed of light-colored clay and sandy clay between 60 and 90 feet above the railroad, and above this within a few feet of the top, sand and fine gravel.

There is however one "kame"-like feature. Upon the top of the terrace (here about 510 feet above mean tide), near the inner end of the gorge there is an isolated knoll about 30 feet high, and of rounded form, which has many cobble stones over its surface, some of them 10 inches in diameter—indicative of

cobble-stone beds within. It has no continuation north or south. The material of the plain around is sand or fine gravel, like that of the upper part of the section. The following figure shows the position of the gravel-made knoll, the form of the

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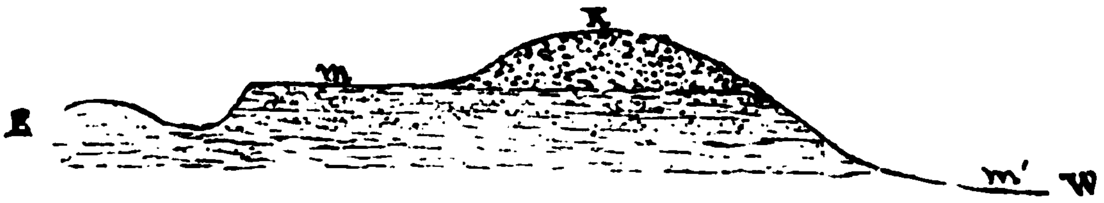


surface north and south, and a section of the beds which according to the facts in the gorge, underlie it. The material of the knoll at top is manifestly the *latest* of the terrace-deposits.

The beds below the level of the railroad were not exposed to view at this place.

The second *east-and-west section* occurs about a fourth of a mile farther north. A long and deep gorge here cuts through the deposits of the 520-foot terrace, nearly to the level of the river, intersecting the "kame" line and extending nearly half a mile to the westward. There is less of kame-like features here than in the preceding gorge. Along the bottom of the deep cut, where a stream flows in some seasons, lay pebbles and some cobble-stones, derived from layers below the level of the railroad track, and these continued for about 300 yards west of the railroad. At a higher level the material is sand or very fine gravel, and the latter in some parts at the top. The sides of the cut were mostly covered by the fallen sands, so that the existence or absence of beds of clay could not be ascertained. A unity of structure from east to west was manifest. Nothing answered to the description of a kame; all was apparently of the terrace formation.

Hanover, New Hampshire, four miles north of White River Junction.—In the town of Hanover, a "kame," according to Upham's map, borders the Connecticut for three miles, to a point north where the river makes an abrupt bend, and thence it follows in the same direct line, the *western* or Vermont side of the river in the towns of Norwich and Thetford, nearly to Thetford village, making in all a length of about seven miles.

The only section of the Hanover "kame" which I have personally examined, is that on the road side between the bridge and the village—the one figured by Upham on page 39 of his Report. At this place, the riverward portion of the stratified drift, or that spoken of by Upham as the "kame," is separated from the following portion by a depression produced by undermining and a dropping of great masses to a lower level, and consequently there are at this place two bluffs, the western which is that of the so-called "kame," and an eastern, which is referred by Upham to the ordinary terrace-formation. In his

figure of the section of the kame here exposed to view, it is made to consist of somewhat arched beds, with alternations of coarse stony layers and finer material alike from top to bottom. I found the bedding horizontal, like that of the eastern of the bluffs; its beds, composed largely of sand and fine gravel, with but few of cobble-stones; and the top portion made of very fine sand, identical in its light color, fine straticulation and other features, with the top portion of the eastern bluff. The latter bluff differs in consisting throughout of stratified sand, and this difference between the deposits near the river and those more remote is not uncommon.

Prof. O. P. Hubbard, of the Medical School of Dartmouth College at Hanover, and formerly Professor of Chemistry and Geology in the Academic Department, has obtained for me the following additional facts respecting the region of the supposed "kame."

He states that no coarse gravel or cobble-stone beds exist along the top of the "kame" *south* of the above mentioned section for the half mile to Mink Brook, and none *north* of the same for nearly a mile, so that this kind of evidence as to the existence of a "kame," fails in these portions. Farther north, above the village of Hanover, there is on the "kame" ridge an area of cobble-stones, and two to three hundred yards beyond this, across a deep cut leading to the river, a grass-covered knoll made up of coarse gravel and cobble-stones, some of the stones a foot or more in diameter. The knoll was found by measurement to be fifteen feet high above the terrace-plain; it marks the spot which is made by Upham, the highest part of the kame, 556 feet above mean tide level. Prof. Hubbard ascertained with a spade that the knoll was composed of coarse gravel, and *rested on* fine sand or sandy loam like that which makes the top portion of the terrace-formation between there and the village and also at the bluffs described above and elsewhere. He concluded, therefore, that the coarse cobble-stone deposit was but 15 feet thick; and, from the level of the other cobble-stone area, that the latter corresponded in position to the lower portion of this deposit. In the deep cut between the two cobble-stone areas the beds are not exposed, but no stones show themselves, and the material was evidently of the same fine sandy nature. Just south of the more southern area three large excavations have been made on the east side of the "kame" ridge to its top, for filling a bog, and these show only sand; but the northern is so near the cobble-stone layer that some of the stones have fallen into it. The evidence obtained by Prof. Hubbard thus appears to prove that the coarse gravel of the two areas is only the top deposit of the terrace-formation, such as characterizes in many other places its riverward portion.

Norwich, Vermont.—The continuation of the Hanover "kame" northward along the west border of the Connecticut in Norwich, passes, near the end of its second mile, the valley of Pompanoosuc River. About a mile south of this turbulent stream, a road ascends from the borders of the Connecticut River to the summit of the high terrace, crossing the "kame" where its height is 565 feet (Upham), half a mile south of the highest point, 600 feet. Along the road are sections of the deposits, showing the inner nature of the Norwich "kame." Where the road commences the ascent some cobble-stones lie scattered over the surface, such as had been found common along the road at the base of the "kame" for the half mile or more to the south. Above this, for the next hundred feet, there is sand, finely straticulate, with occasional fine gravel. Nearing the top, the beds become coarse gravelly, and then there are large cobble stones; and this upper coarse-gravel portion rises above the general level of the plain, making a low ridge which is the crest of the so-called "kame." In the higher part, to the north, some stones, as stated by Upham, are 4 to 5 feet in diameter and angular.

Nothing was observed on the ascent from the river, or on the west side, to suggest a suspicion that this cobble-stone deposit was the top of a narrow range of coarse gravel beds buried beneath the terrace-formation; on the contrary, the evidence from the sections along the ascent, and especially the succession of beds toward the top from sand beds to gravel beds, and then to the coarse cobble-stone gravel, strongly confirmed the natural inference that all was one consecutive series, with the cobble-stone deposit the uppermost and therefore the latest. West of the cobble-stone ridge, or the "kame," the terrace has great extent. The surface falls off immediately 40 feet, exposing the materials that lie beneath, and these are sand and fine gravel as on the east side.

The Pompanoosuc river was probably the chief source of this coarse material of the summit. To the southwest, about the village of Norwich, the terrace is quite stony over much of its surface from the contributions to the terrace of Blood Brook.

In Thetford the "kame" becomes very low before the village is reached.

The other reputed "kames" of the Connecticut River valley I have not particularly examined, But as the line from Windsor to Thetford is "the kame of the Connecticut valley," essentially "a continuous gravel ridge or kame, extending 24 miles," and is made, in Mr. Upham's work, the text for the description of "kames" in general, details from the other minor "kames" in the valley are not necessary for a right conclusion.

Conclusion.—The conclusion from the investigation is, as already indicated, the following: that the supposed "kames" are portions of the terrace-formation, with which they usually correspond approximately in height; and that their materials were the same in source with the rest of the stratified drift, and the beds the same in time of origin.

The gravelly character of the terrace-formation off the mouths of the tributaries of the Connecticut is often mentioned by Mr. Upham; and, if the above conclusion is right, the coarse material of the "kames" is to be explained on the same principle. The position of these coarsest deposits, near the borders of the flooded Connecticut, whether they make the lower or the upper beds, is a consequence of the rapid flow of the waters in this great stream, which drifted away much of the finer material within reach and left stones. The coarsest stone beds at the mouth of White River are located where the two streams—both great streams then—join, that is, where the great contributor of gravel and stones encountered the great distributor.

The deposit of gravel and stones in the upper portion of a terrace I have attributed to the violence of the flood when at its maximum stage. But in the region of the so-called "kames," from Windsor to Thetford and beyond, floating ice was probably needed for much of the transportation; and ice-floes would have been abundant at the time, when the glacier-ice was in rapid process of dissolution about the slopes of the Green Mountains—the range at the head of the principal tributaries in this part of the Connecticut valley. At the same time, the Connecticut, by its rapid flow along its eastern side at one time and its western at another, would have determined an accumulation of stony material along its borders, as a great river now produces accumulations on its banks different from those more distant. Here the floating ice with its burden of earth and stones would have been stranded as well as other transported materials. Moreover such deposits might have been raised ten feet or more above the plain adjoining, as now happens on large streams from modern floods. But there is no occasion to account for a cobble-stone deposit along the whole top of any of the so-called "kames;" for, only a small fraction of each has a crest of this kind; or any difference in structure from the ordinary terrace-formation, except that in some cases, near tributaries, they have more of coarse gravel below.

In Haverhill the angular stones and gravel, brought down the Ammonoosuc on ice-floes, made in one place a thick till-like deposit lying unconformably over the stratified drift and continued some distance down the riverward slope of the terrace. This is an exceptional case, due probably to the fact that the White Mountains, the source of the stream, are near by.

But the ridge-like feature of many of these coarse upper deposits, on the riverward part of the terrace-formation, that is, their standing up 15 to 60 feet above the level of the terrace around, and sometimes higher, is in part, if not chiefly, due to erosion. The Norwich stony deposit, on the top, south of the Pompanoosuc, has a large and broad depression west of it; and so has that south of White River Junction, that of Hanover, and others. Even the little knoll described on page 463 has its adjoining depressions, as shown in the figure there given, and the gulch descending from the southern one of these depressions may be a further consequence. The waters of rains, making rills or streamlets, easily remove the sand and fine gravel of the terrace-formation; but they make comparatively little impression on the beds of coarse gravel and cobble-stones, because of the size of the stones and often also their partial consolidation by iron oxide (limonite). Hence the waters which fall over the stony surface find a place of descent and wear away on either side; and with every new inch of descent gained there is a gain in fall and force, and a quickening of the work of erosion. The channel begun is deepened and widened, waters from the plain flowing in and helping in the removal: and thus broad channels like river-channels may form over wide plains, and deep gorges be cut through to their depths if a place of discharge is at hand. Besides, the river at the time of greatest height swept over the terrace plains with often 40 to 60 feet or more of depth, and large denudation in some parts would have been the consequence.

The above explanations have reference to those so-called "kames" examined by me in the Connecticut River valley. I make no sweeping application of them to those which have been described from other regions that I have not seen. It was my purpose to have studied, the past season, also the terraces of the Merrimack valley, but time failed me.

The gravel ridges of the vicinity of Andover, Massachusetts, first described by Prof. E. Hitchcock, and lately studied with care and designated "kames" by Prof. G. F. Wright, appear to represent a phenomenon of a different class. I had the guidance of Prof. Wright in a day's excursion over them, and was led to think, as he does, that these isolated ridges of unstratified coarse gravel and stones are of morainic sub-glacier origin; and, perhaps, lateral. though sub-glacier, moraines, left between bodies of ice that moved southeastward along the depressions—now marsh-filled—which exist either side of them. But without more study of them, and especially of their relation to the deposits of the Merrimac valley, I would not express a decided opinion on the question.

Nothing has here been said with regard to the "kettle-holes," that is, isolated kettle-shaped and often pond-containing depressions, which, in Mr. Upham's view, were connected in origin with the "kames;" and for the reason that they occur also over ordinary terrace-plains. Further, Mr. Upham's hypothesis as to the origin of "kames" there is obviously no occasion here to discuss.

Some points in the explanations above advanced need, in view of the difference of opinions among writers, further consideration, and will be made the subject of another communication.

ART. LX.—*Japanese Seismology*;* by Professor C. G. ROCKWOOD, Jr., Princeton, N. J.

THE change in the foreign policy of the Japanese, by which that country was opened to the influences of western civilization, gave an impulse to several branches of scientific investigation for which Japan affords special facilities; but in no department has there been more hopeful progress than in the study of Seismology.

The opportunities for the development of this science in Japan are exceptionally good. Earthquakes are here quite frequent, averaging for the whole kingdom more than one every day, and sometimes far exceeding that number. Hattori has found native records of 817 shocks in the fourteen months from Nov. 1, 1854, to Dec. 31, 1855. The earthquakes also are mostly of moderate intensity and therefore better fitted for instrumental study than those violent and destructive convulsions which leave their record in ruined cities and decimated communities. The centers of learning and science, where are naturally gathered the greater number of persons qualified and disposed for such investigations, are on the shores of Yedo Bay, a district specially subject to earthquake shocks and whose geological character is tolerably well known. Here, in the capital Tokio, a society has been formed for the especial study of Seismology, including in its membership professors, both native and foreign, from the educational institutions of the city, having as its president a native Japanese, I. Z. Hattori, A.B. (Rutgers), and for its vice-president Professor John Milne; and which has printed, as the result of its first year's work, a volume of Transactions amounting to 188 octavo pages. Accounts of the work done in this society and contributions from its members on topics related to Seismology are also published from time to time in the Japan Gazette.

* Read before the Princeton Science Club, Oct. 27, 1881.

In directing attention to those who have labored in this field, we have to mention the names of E. Naumann, John Perry and W. E. Ayrton, I. Z. Hattori, W. S. Chaplin, E. Knipping, J. A. Ewing, G. Wagner, T. Gray and John Milne, all of whom have added to the available stores of information, by the examination of native records, or by the invention and improvement of instrumental appliances.

In the literature of Japan are found numerous accounts of past earthquakes, reaching back even to 295 B. C., at which time it is recorded "Fujiyama was upheaved." These native records have been examined by Dr. Naumann,* Mr. Hattori,† Mr. Knipping,‡ and Professor Milne,§ and have furnished abundant material for discussion. Indeed the amount of Japanese Seismological literature is unexpectedly large. Dr. Naumann mentions the titles of thirty-three and Hattori o thirty-four native books consulted in preparing their papers, while Milne is acquainted with sixty-five native earthquake books besides seven earthquake calendars. A part of this earthquake literature, especially the calendars, has a scientific value, but on the other hand much of it is made up of a series of anecdotes often of a trivial character. For illustration of these, a single one, selected from an account ‖ of the great shock of 1707, will suffice.

"How an impetuous man fell down from up-stairs."

"Five or six young men were singing and drinking up-stairs in a tea house in Horiye. In the midst of their happiness they were suddenly alarmed by the earthquake and at once became bewildered. While one of them was looking out he missed his footing and fell down from the ladder into a konomono-oke (a cask containing radishes pickled in salt and bran which is very offensive to the nose). The others who were yet up-stairs intended to come down. But the man in the cask looking up said that below all was chaos and it would be better to remain up-stairs. The reason why the man below said that all was chaos was because he had not perceived that it was by accident that he had fallen into the cask."

The earthquakes contained in Naumann's and Hattori's lists have been discussed by their authors and by Ayrton,¶ with

* Ueber Erdbeben und Vulcanausbrüche in Japan. Mittheilungen der deutschen Gesellschaft für Natur- und Völkerkunde Ostasiens. 15tes Heft.

† Destructive Earthquakes in Japan. Transactions of Asiatic Society of Japan, vol. vi, p. 249.

‡ Verzeichniss von Erdbeben, wahrgenommen in Tokio, von Sept. 1872 bis Nov. 1877. Mittheilungen der deutschen Gesellschaft, etc., Ostasiens. 14tes Heft.

§ Japan Gazette, June, 1881.

‖ Milne in Japan Gazette, 1881.

¶ Note on the Periodicity of Earthquakes in Japan. Transac. Asiatic Soc. of Japan, vol. vi, p. 320.

respect to the seasons, the motions of sun and moon, the frequency of sun-spots, meteors, etc.; and Professor Chaplin * has examined in the same way the records for three years (1875–8) of the Palmieri instruments in the Meteorological Observatory of Tokio. But the results are entirely negative, not confirming Professor Alexis Perry's deductions from a similar examination of his lists, although Hattori and Ayrton both think they find some indications of a periodicity in destructive shocks.

Besides examining native records, much attention has been given to the instrumental investigation of the earth-motion. In this work Perry and Ayrton, Wagner, Chaplin, Ewing, Gray and Milne have all had a part.

The devices suggested by former observers have been here tested anew. Pendulums long and short, suspended and inverted, with bobs light and heavy, and making their records by scratching a smoked plate, by pushing light rods arranged against them, or by pulling cords and turning pointers over graduated arcs, the fluted mercury dish of Cacciatore, the graduated cylinders of Robert Mallet, and the bent tubes and loaded springs of Palmieri, as well as the microphone suggested by Rossi have all been employed and have done good service.

But no one of these was entirely satisfactory. Not to mention other difficulties, the pendulums and loaded springs had each a normal rate of vibration, and were ready to take up and accumulate earth vibrations of similar rate, while remaining to a considerable extent unaffected by those of a different period. So that the records of the earth-motion were complicated or perhaps entirely concealed by those due to the normal vibration of the apparatus. This difficulty, long known, was stated and mathematically discussed by Perry and Ayrton in a paper read before the Asiatic Society of Japan in 1877 and afterward published.† The remedy suggested by them was to support a heavy ball within an iron box, by spiral springs of such stiffness as to make its normal rate of vibration much quicker than any ordinary earthquake wave.

Moreover, while these instruments of former observers gave some more or less accurate indication of the time of an earthquake shock, and of its direction of propagation and relative intensity on some arbitrary scale, they afforded very little knowledge of the extent or character of the actual motion of an earth-particle, and to this end especially has tended the instrumental work of Japanese investigators. In this direction

* Examination of the Earthquakes recorded at the Meteorological Observatory of Tokio. Transac. Asiatic Soc. of Japan, vol. vi, part II.

† On a neglected principle that may be employed in Earthquake Measurements. London Phil. Mag., V, vol. viii, p. 30, July, 1879.

was the attempt by Dr. Verbeck in 1873 to support a heavy planed block of wood upon four crystal balls, these resting upon a polished marble slab carefully leveled. The block was then in neutral equilibrium and a pencil attached to it would leave a record of the motion upon a paper fastened to the slab beneath. Such a record was found to be too minute to be of service, and an important aim of later devices has been to procure in some way an *enlarged* record of the earth motion. This has been accomplished, in two ways: by employing an indicating lever with unequal arms, the shorter arm being acted on by the motion of the earth, while the longer arm carries the writing style which makes the record, or by causing the earth-motion, through the medium of a fine cord, to turn a small pulley to whose axis is attached a long light pointer.

The earliest apparatus by which a magnified record was obtained was Wagner's Pendulum Seismometer, first described in a paper read before the German Asiatic Society of Tokio in June, 1878, and printed in the Transactions of that Society. After two years experience a full description of the apparatus was published in the Japan Gazette (July 10, 1880).* It consists of an iron ball weighing forty or fifty pounds, suspended by a bundle of silk threads three feet long. At the moment of a shock this heavy ball by its inertia remains stationary. Beneath the lowest point of the ball, a light vertical indicating lever or pendulum is supported by a bar rigidly connected with the earth. The fulcrum of this indicating lever is formed by a metallic sphere $\frac{3}{8}$ inch in diameter, on which it rests by a smooth plate forming the top of a short hollow cylinder of the same internal diameter as the metallic sphere. The point about which this lever pivots is therefore the center of this small supporting sphere. The upper end of this lever, the shorter arm, engages with a similar small sphere attached to the lower part of the heavy iron ball; while the lower and longer arm is attached to a light thread that passes through a hole in a porcelain plate. Of course any motion of the ground is transmitted to the support of the indicating pendulum and causes relatively magnified motion of the lower end of the same the amount of which is indicated by the length of thread drawn through the hole. It appears to the present writer that the elasticity of the silk cords supporting the heavy ball would introduce an element of uncertainty into the indications of this apparatus, as quantitative results could be hoped for only on the assumption that there was no *vertical* motion of the heavy ball with respect to the support of the indicating pendulum. Of course this seismometer gives indication of the amount of horizontal motion only. The *direction* must be obtained from other

* Transactions Seismological Society of Japan, vol. i, part I, p. 54.

apparatus used in connection with this, as must also the vertical component.

Another device for obtaining a magnified record of the earth-motion is Gray's Rolling Sphere.* This consists of a heavy lead or iron sphere resting in neutral equilibrium upon a level plane, and therefore free to roll in any direction. Above the sphere an indicating lever is supported in a vertical position, by a sort of spring universal joint, so that its lower extremity, the shorter arm of the lever, engages with a hole in the highest part of the sphere, while its upper and longer arm carries the recording style. The method of arranging the fulcrum of this lever is peculiar. The light rod forming the lever passes centrally through a small disk to which it is fastened. This disk plays within a horizontal ring, from which it is supported, through the medium of four bent springs, which are attached by one end to symmetrical points on the ring and by the other to the edge of the disk. The lever has a small weight on its lower arm sufficient to bring the center of gravity below the fulcrum and to make its normal rate of vibration slower than that of the earthquake. The lever supported in this way is, by the elasticity of the springs, free to move in any direction as influenced by the motion of the heavy sphere.

Gray's Double Bracket Seismograph † also gives a magnified record of the actual motion of an earth-particle. This consists of a post planted firmly in the ground, to which is hinged, by its longer side, a light but strong frame, something like a gate, measuring 60 × 15 centimeters. The upper hinge is a knife edge in a ring, while the lower is a point resting in a conical socket. To the outer edge of this frame is hinged in the same way another similar but somewhat lighter one, loaded on *its* outer part by a thick metal disk of considerable weight, which by virtue of its inertia forms the stationary point of the seismograph. When ready for use the planes of these two brackets are placed at right angles to each other, and each makes an angle of forty-five degrees with the face of the post. The record is made through the medium of an indicating lever similar to that above described with the rolling sphere, and supported below the center of the heavy disk by an arm extending out from the post.

Gray's Pendulum ‡ Seismometer aims to record the earthquake motion by means of its components in *three* directions at angles of 120°. It consists of a heavy weight hanging by a cord three feet long, from the middle of a stretched wire. It is

* On Instruments for Measuring and Recording Earthquake Motions. London Phil. Mag., V. vol. xii, p. 199, Sept., 1881.

† London Phil. Mag., V. vol. xii, Sept., 1881.

‡ Transactions of Seismolog. Soc. of Japan. vol. i, part I, p. 44; also Lond. Phil. Mag., V. vol. xii, Sept., 1881.

thus able to move in a vertical as well as a horizontal direction, and the amount of this vertical motion is recorded by attaching to the upper part of the pendulum a fine thread which turns a small pulley above and thereby moves a long pointer. To register the horizontal motion, three radiating cords pass from the center of inertia of the heavy bob of this pendulum to three horizontal pulleys to which are attached long pointers that magnify the actual motion twenty-five times. The method of attaching these pointers to the pulleys is new and ingenious. The pointer is hung to the under side of the pulley by a bifilar suspension, so that there is no tendency for the inertia of the pointer to carry the pulley too far, as was found to be the case if they were rigidly attached. The inventor proposes to prevent the pendulum from accumulating earth vibrations that may happen to synchronize with its own normal rate, by allowing a pointed rod to slide on a glass plate below the bob and weighting this sufficiently to produce the necessary friction.

In Ewing's* Pendulum Seismograph this same object is accomplished by using a pendulum twenty-one feet long, so that its normal time of vibration is about five seconds, much longer than any earthquake vibration. The bob of this pendulum, which has been erected in the University of Tokio, is a cast iron ring, whose section is 2 × 4 inches and internal diameter 16½ inches, and which is suspended in a horizontal position from the top of a firmly braced framework. This heavy ring is crossed by a diametral bar, at the middle of which are applied the short arms of two bent levers, whose long arms mark upon circular smoked glass plates, caused to revolve continuously by clockwork. The planes of these two bent levers are placed at right angles to each other, and they are supported by gimbal joints in such a way that each is affected by motion in one direction only. The horizontal motion of the pendulum bob is thus separated into two rectangular components which are recorded separately.

Another device which records the two components of the earth motion separately is Gray's† Rolling Cylinder Seismograph. Here a pair of exactly similar hollow cylinders of metal are placed on a smooth level plane, with their axes horizontal and at right angles. Being thus in neutral equilibrium they are free to roll, and their motions are recorded by the magnifying levers whose fulcrums are upon a fixed support above the cylinders and whose long arms write upon a moving drum or plate.

Still another arrangement, the Bracket Ring Seismograph, which has already done good service, is a modification of Zöll-

*A new form of Pendulum Seismograph. Transactions Seismolog. Soc. of Japan, vol. i, part I, p. 38.

† London Phil. Mag., V, vol. xii, Sept., 1881.

ner's horizontal pendulum, due originally to Chaplin and improved by Ewing and by Gray. It consists essentially of a weight supported on a horizontal bar, which is attached at one end to a vertical axis and at the other end carries a long pointer writing upon a moving plate. This will of course record only one component of the motion, viz: that at right angles to the direction of the pointer, and such apparatus must be used in pairs placed at right angles to each other.

The apparatus for vertical motion, which was used in connection with this, was a vessel of water supported from above and having a flexible bottom, which would be acted upon by the inertia of the liquid and would make its record by a multiplying lever upon a moving plate.

A modification of the conical pendulum by Gray * promises to afford a very sensitive seismograph but it cannot well be described without diagrams.

Milne's tremor indicators † are also most delicate and sensitive. From a rigid frame is suspended, by a short wire, a heavy mass, against the sides of which rest two small horizontal bars of wood. Under the outer end of each bar a small mirror is hung by a bifilar suspension, one thread to the bar and the other to an adjacent fixed point. Then any motion of the heavy mass relative to its support causes motion of the bars, and so of a beam of light reflected from the mirrors. A motion of $\frac{1}{10000}$ of an inch is readily detected in this way.

Numerous other devices are described in the Transactions of the Seismological Society of Japan and in the other papers above referred to, and to these sources the reader is referred for further information in regard to them.

We are now to consider some results obtained by the use of instruments and the discussion of their records. The published volume of Transactions of the Seismological Society, Part II, contains from the pen of John Milne, a long account, amounting to over one hundred pages, of the earthquake of February 22, 1880. It is based on one hundred and twenty written communications received by the author, of which thirty were detailed replies to a series of printed questions. Our limits forbid anything more than a brief statement of a few selected points.

The direction of the shock was deduced from personal reports and from the indications of Palmieri's instrument, a Cacciatore and a pendulum recording its motion on a smoked glass. The general result was that there had been *two* shocks, the first in a direction approximately to or from N.N.W., the

* London Phil. Mag., l. c.

† See paper "On Recent Earthquake Investigations," by T. Gray in Chrysanthemum, vol. i, No. 5, May, 1881.

second N.N.E. or N.E. It is interesting to note that the pendulum records of the Luzon* earthquake of July, 1880, show likewise the presence of several wave directions in azimuths not widely different from those here stated.

Again, not only were many chimneys and similar objects overturned, but in numerous instances chimneys and monuments in the cemeteries were twisted upon their bases, sometimes through an angle of 20° or 30°, without being overthrown. The rotation was usually but not invariably in a direction contrary to that of the hands of a watch. As to the cause of such rotation, Mallet's explanation, which attributes it to the vertical through the center of gravity not coinciding with the center of friction, is rejected as not in accord with the great preponderance of rotation in one direction, and another explanation suggested by T. Gray is offered, to this effect: If any columnar object having a rectangular base is acted upon by a force parallel to either side or to either diagonal of the rectangle, it will tend to overturn without rotation. If, however, the force has any direction other than these, there will be a tendency to rotation in a direction determined by the relation of the line of force to that diagonal which lies nearest to it. If the rectangle be divided into eight equal triangles by the two diagonals and two medial lines parallel to the sides, and the alternate triangles be shaded, it will be seen that the rotation will be in one or the other direction, according as the direction of the force, falls in a shaded or an unshaded triangle. The direction in which a stone is found to have been twisted will then enable us to assign limits to the direction from which the impulse that moved it must have come, and will thus serve to indicate the direction of the earthquake shock. With regard to the earthquake in question, the direction inferred in this way from the numerous twisted grave-stones, agrees in general with the instrumental indications noted above.

This earthquake was sensibly felt over an area included within a radius of one hundred and twenty miles. From the directions of the shock as observed at Tokio and Yokohama, and from other considerations, the author concludes that the probable origin of this earthquake was nearly equidistant from Tokio and Yokohama, but somewhat to the east of them, under the eastern shore of Yedo bay. Indeed many of the recent earthquakes in Japan seem to come from that region. The geological characteristic of that district is beds of volcanic tufa and breccia very much faulted and contorted in the southern part and giving evidence of recent elevation. It is in the prolongation of a long line of volcanoes and volcanic islands, extending 1,500 miles southward into the Pacific through the Bonins; and it is also on another line of volcanoes, 3,000 miles

* This Journal, III, vol. xxi, p. 52, January, 1881.

long, extending from Kamschatka to the Philippines. The suggestion is ventured then that this and other earthquakes are to be attributed to action taking place about the end of the fissure in the earth's crust, marked by the first mentioned line of volcanoes, of which Ooshima, sixty miles south, is the nearest active vent; "that this line is still endeavoring to open for itself vents still farther north;" and "that beneath Yedo bay there is a point where volcanic agencies are endeavoring to force a way."

These conclusions as to the probable origin of the frequent earthquakes are further confirmed by later observations of two different sorts.* Prof. Milne in Tokio. and Mr. W. H. Talbot in Yokohama, have made careful time observations, using clocks with sensitive apparatus to stop them at the instant of a shock and keeping the clocks regulated by daily telegraphic comparisons. The result is that the shocks are usually felt in Yokohama from fifteen to thirty seconds earlier than in Tokio, indicating an origin nearer to the former place. Again seismometers were placed at Tokio, at Yokohama and at Kisaradzu on the opposite side of the bay, with special reference to determining the direction of the shocks, and gave the following results. On Jan. 7, 1881, the directions intersect within *two* miles of Yokohama; on Jan. 22, 1881, the intersection was *four* miles south-southeast of Yokohama; and on Jan. 24, 1881, the intersection was *seven* miles south-southwest of the same place,—again all indicating an origin near Yokohama.

But perhaps the most interesting of recent results was obtained from the earthquake of March 8, 1881, some notes† on which were read before the Seismological Society on March 23d, by Prof. Milne. At this shock a complete record of the earth-motion for over twenty-five seconds was secured. The instruments used were a pair of "bracket ring" seismographs, writing upon a slip of smoked glass, for the two horizontal components, and a water vessel with flexible bottom for the vertical component. The bracket-ring machines (No. 1 and No. 2), were purposely placed so as to record vibrations at right angles to and in the direction of a line joining Tokio and Yokohama (S. 23° W).

No. 1 showed a decided motion, there being about seven vibrations in five seconds, or one complete vibration in $\frac{5}{7}$ of a second. The greatest indicated motion in this direction is about 1·3 millimeters.

No. 2 indicated very slight but sensible motion.

No. 3 for vertical motion showed about six distinct waves in a space indicating twenty-five seconds of time.

These records, confirmed as they are by the register of Palmieri's instrument and of eleven different pendulums, show

* Japan Gazette, Feb. 5, 1881.

† Japan Gazette, April 2, 1881.

clearly that the main vibration in the vicinity of Tokio was in a general east-and-west direction. The time observations, and other considerations also, indicate that the origin of the shock was in the faulted region near Yokohama. Hence Milne is led to the conclusion that the vibrations observed were *transverse* to the direction in which the wave was moving, instead of normal as usually supposed; and that the wave, at least by the time it reached Tokio, was one of distortion, not of compression. It is probable that in any ordinary earthquake both sorts of wave are coëxistent, at least near its origin; but experiments made by Milne upon artificial shocks produced by the fall of a heavy weight, tend to show that the transverse vibrations are the more persistent and are felt to a greater distance than the longitudinal. Moreover, if the earthquake wave originated by the tearing open of a fissure in the rock and the sliding of the surfaces upon each other, a shearing force would be exerted which *might* produce a wave of distortion without any accompanying wave of compression.

The Japan Gazette of July 23, 1881, contains a note of some interesting observations on an earthquake of July 5, 1881, showing that the motion of the ground varied considerably in *direction* during the same shock. The records were made by Gray's Double-bracket Seismograph writing upon a smoked plate. Prof. Milne says:

"Near to the commencement of the shock the motion was N. 112° E. One and a half seconds after this the direction of motion appears to have been N. 50° E. In three-fourths of a second more it gradually changed to a direction N. 145° E.; and after a similar interval to N. 62° E. Half a second after this it was N. 132° E., and four seconds later the motion was again in the original direction, viz., N. 112° E. There appear to have been at some portions of the shock not more than four vibrations per second, at other portions there may have been as many as ten. The greatest amplitude of motion does not appear to have reached one millimeter."

The records of the various instruments agree in the indication that the amplitude of vibration of an earth-particle, at least in such shocks as ordinarily occur in Japan, is much smaller than has generally been supposed, not more than a very few millimeters. Of twenty earthquake shocks observed by E. Knipping* with Dr. Wagner's apparatus only two exceeded 2·5 mm. in amplitude, and a similar fact has been incidentally mentioned in respect to several of the earthquake shocks spoken of above.

To conclude our review of what has been done in Japan in this department of research, the results achieved can perhaps best be summed up in the words of Professor Milne himself in

* Transactions Seismolog. Soc. of Japan, vol. i, part I, p. 71.

his report to the British Association at its recent meeting in York, where he states them thus:—

"1st. The actual back-and-forth motion of the ground is seldom more than a few millimeters (usually not equal to one millimeter) even though chimneys have fallen.

"2d. The motion usually commences gently but is very irregular.

"3d. The number of vibrations per second usually varies between three and six.

"4th. During one shock the *direction* may be irregular.

"5th. East and west vibrations as recorded at Yedo (Tokio) have in some cases been shown by time observations to have traveled up from the south.

"6th. Many of the shocks which visit Yedo appear to have come from a district which is much faulted, and which gives evidence of very recent elevation."

This brief and no doubt incomplete survey of the field considered gives reason to believe that the knowledge of the phenomena and causes of earthquakes has received and will receive important additions through the labors of these residents of the far east; and that this youngest of the scientific societies of Japan, whose exhibition of seismographical instruments attracted 2,000 visitors in one day, has such a hold upon the interest of that community that it will not be left without support even though all its foreign members should be withdrawn from the country.

In conclusion I desire to say that for much of the information embodied in this paper I am indebted to the kindness of Prof. John Milne of the Imperial College of Engineering in Tokio.

NOTE.—Since this paper was written, the November number of the London Philosophical Magazine has come to hand, containing an article of 22 pages by John Milne and Thomas Gray, on "Earthquake Observations and Experiments in Japan." It is a *résumé* of work done by the authors during their residence there, and consists of two parts, the first devoted to a description of the instruments used, the second to a discussion of the Earthquake motion. The instruments are described under the head of 1. Seismoscopes; 2. Seismometers and Seismographs; 3. Instruments for vertical motion; 4. Apparatus on which to record earthquake motions; 5. Time-takers. In the second part, the authors discuss the relation of the normal and transverse vibrations, the details of the movement as illustrated by a copy of the instrumental record made by a pair of conical pendulums on July 25th, 1881, the relative frequency of earthquakes at different seasons, the effect on buildings, and the rotation of bodies. C. G. R.

Princeton, Nov. 17, 1881.

ART. LXI.—*An Apparatus for the Distillation of Mercury in Vacuo*; by ARTHUR W. WRIGHT.

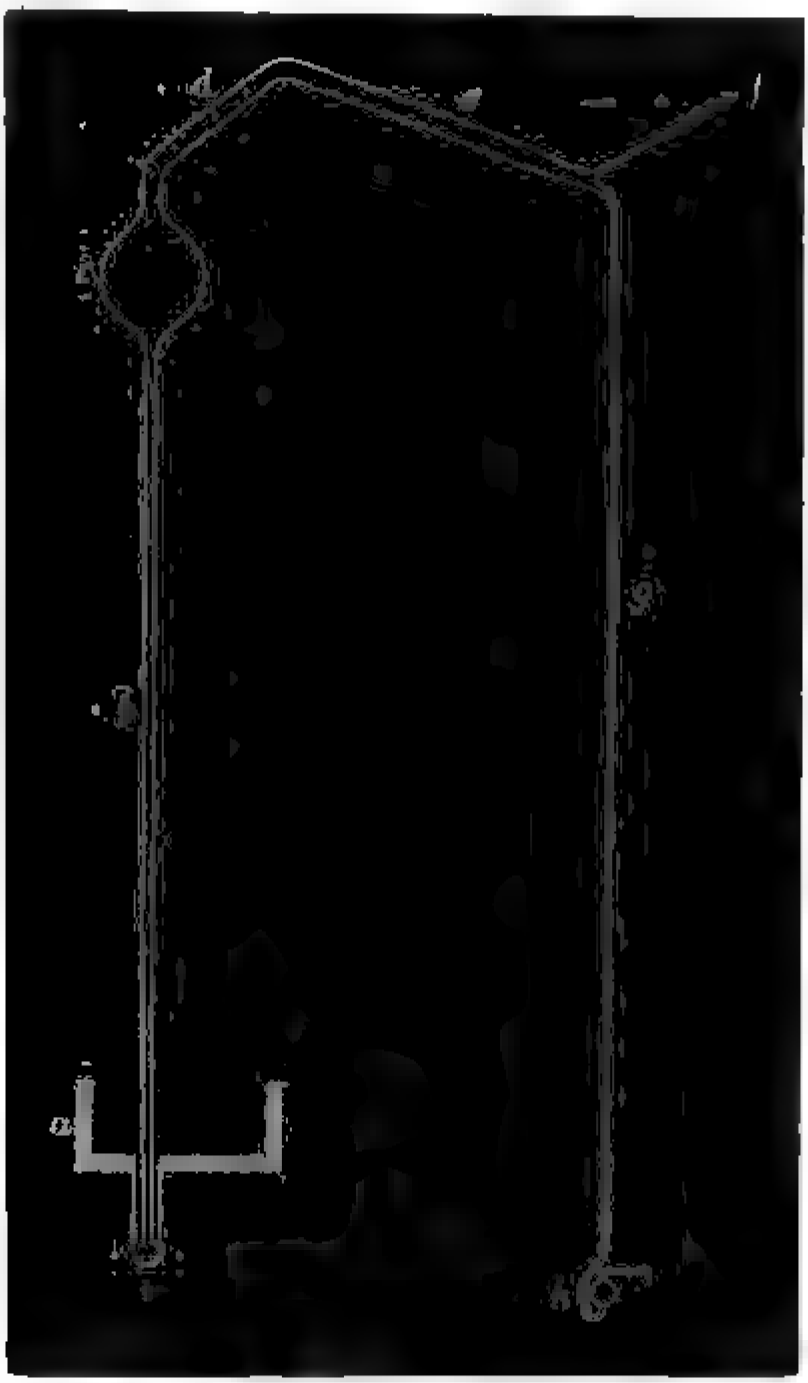
THE importance of pure mercury in many of the operations in the laboratory makes a simple and efficient means of freeing from its impurities the ordinary commercial metal, or that which has become fouled by use, an object greatly to be desired. The familiar chemical methods, aside from their inconvenience, are not entirely satisfactory, and often leave the condition of the product uncertain. Distillation in the usual way, in retorts open to the air does not prevent contamination by oxidation, and the purity of the metal is further endangered by the liability to spurting and the possible presence of substances volatile at the boiling point of mercury. When the process is conducted in a vacuum, however, these drawbacks are avoided, and a perfectly pure product is obtained.

A very elaborate and complete apparatus for this purpose has been devised by Professor Weinhold,* which fully satisfies all the conditions of the problem. This instrument has provisions for the maintenance of the vacuum by means of a Sprengel pump which constitutes a special part of it, with suitable arrangements for adjustment of the mercury supply, the heat from the gas burner, and the like. The devices for securing these objects, however, render the apparatus somewhat bulky, and complicated in structure. A far simpler construction has been employed by Dr. L. Weber,† which however has no contrivance for maintaining or renewing the exhaustion, except by refilling with mercury, and otherwise leaves much to be desired. Its consists essentially of a long glass tube bent into a U-shape so that when filled with mercury and inverted with the ends of the tubes in vessels containing mercury it forms a double barometer, the bend of which is above the level of the metal and therefore vacuous. An enlargement at one side where the heat is applied by a small Bunsen flame gives an increased surface of evaporation. The mercury vapor condenses in the upper portion of the empty space and flows out through the other branch of the tube.

The apparatus devised by the writer is based upon Weber's plan of a double barometer tube, but with important modifications which secure substantially the advantages of the more complicated system of Weinhold. The most essential portions of it are represented, in section, in the accompanying sketch, which is drawn to a scale of one-tenth that of the instrument

* Carl's Repertorium für Physik, vol. xv, p. 1. † Ibid., vol. xv, p. 52.

itself. The principal member of the still consists of a single continuous piece of glass work, which, for convenience of description, may be regarded as made up of several distinct parts designated by the letters *b*, *c*, *d*, *e*, *f*, *g*, *h*.



The portion *b* is a straight, rather heavy piece of tubing, of about one centimeter exterior, and five or six millimeters interior, diameter. Its length is a little more than 76 centimeters. It is open at the lower extremity, and at the other is enlarged to an oval bulb, *c*, about 85 mm. in diameter and 120 mm. long. At the upper end of this is joined the portion *d*, *e*, having an interior caliber of about 15 mm. The vertical portion next to *c* is 25 mm., the inclined portion, *d*, 130 mm., and the sloping part, *e*, 300 mm. in length. The object in making *d* so long and giving it the inclined position was to

prevent any globules of mercury thrown up from the bulb entering the portion *e*. But it might well be somewhat shorter, as with proper care in the application of the heat no shocks of the mercury in boiling ever occur.

Toward the end of *e* the glass is narrowed, and, at the angle, it passes to a continuation *g*, which is a straight, vertical tube having an interior diameter of about one millimeter. The angle is so formed that the globules of mercury running down from *e* fall freely into *g* without accumulation at any point. This part of the apparatus is in fact a Sprengel pump, and the mercury as it passes out maintains the exhaustion of the whole tube at a very high point. A small tube, *f*, serves to make connection with the air-pump at the beginning of the operations. The tube *g*, at its lower end, *h*, is bent upward and a small bulb blown upon it, sufficiently large to hold enough mercury to fill *g* itself. Above the bulb the tube is bent into a horizontal direction, this part being 30 or 40 millimeters long, and then directly downward, forming the outlet for the mercury. The total length of *g* is 90 centimeters.

A cistern, *a*, serves for the reception of the metal to be operated upon. It is a wooden box 150 mm. square, and about 60 mm. deep. The joints are carefully fitted and the wood oiled and then well varnished, being thus rendered quite impervious. A small well, 80 mm. deep, for the reception of the end of the main tube, is made by inserting a thick glass tube in the bottom of the box. This arrangement, with the large area of the cistern, increases the range of adjustment of the latter, and makes it possible for several kilograms of mercury to pass through the apparatus before any such alteration of level in *c* is produced as to require a new supply, or a readjustment.

The glass tube and cistern are mounted upon a light wooden frame, the weight of the former with its contents being chiefly sustained by an iron ring which touches the bulb some distance below its widest part. Several layers of fine wire gauze carefully fitted to the lower half of the bulb are interposed between it and the ring, forming an elastic bed, and serving also to distribute the heat. A cylinder of thin sheet copper just large enough to slip through the ring is supported upon the latter by a narrow flange at the top. It extends downward about 60 mm. and is pierced with a number of holes just beneath the ring. It serves to direct the heated current from the burner upon the bulb, as also to protect the flame from movements of the air, and render it steady in its action.

The heating apparatus consists of a hollow ring having upon the upper surface 12 holes 2·5 mm. in diameter, spaced uniformly in a circle of 25 mm. radius, and concentric with the

glass tube which passes through it. The ring is fitted to the top of a common Bunsen burner, and the whole moves upon a vertical slide, with a clamp screw, by which it may be fixed at any point desired. It has been found advantageous in practice to use but six of the openings, and these all upon one side. A conical hood of sheet copper encloses the upper half of the bulb, and is prolonged by a tube of the same material, which covers the glass tube as far as the angle above *d*. The lower edge of the hood is at nearly the same level as the top of the cylinder above mentioned, and is about six centimeters wider than this, so as to project laterally some three centimeters all round. The upper portion of the copper cone and tube are wide enough to leave an interval of five or six millimeters between them and the glass. The heated gases from the holes in the cylinder, streaming through this space, envelop the bulb *c* and tube *d*, thus preventing condensation of the mercury vapor before it reaches *e*.

The cistern is mounted upon a vertical slide with a clamp screw, and can be moved up or down, the range of motion being about six centimeters. This makes it possible always to bring the mercury to the proper height within the bulb, and to suit the adjustment to the varying atmospheric pressure.

The apparatus is put in operation as follows: Connection having been established with a Sprengel air-pump by means of the tube *f*, mercury is poured into the cistern so as to cover the bottom of it to the depth of a centimeter or two. If pure mercury is at hand the bulb at *h* may be filled with it, if not the extremity of the outlet tube is sealed or otherwise tightly stopped. As the exhaustion proceeds the mercury rises in *b*, finally reaching *c*, and if all the air were removed, it would stop at the barometric height above the surface in the cistern. The latter is adjusted so that the top of the column is a little below the center of the bulb, *c*. When no more air can be withdrawn by the pump, *f* is sealed with a gas flame and the connection with the pump severed. The apparatus is thus exhausted once for all, as subsequently it maintains the vacuum by its own operation. The burner, previously set some distance below the bulb, is now lighted and the flame made very small at first. The mercury soon becomes heated, vapor is formed, and after a time drops begin to fall from the interior surface of the bulb and tube above it. The flame is slowly increased and raised, until, in fifteen or twenty minutes, the vapor passes the angle at the top and begins to condense in *e*. As the globules of mercury fall into *g* they carry with them the residue of the air, gradually filling the bulb at *h*, and later the tube *g* itself. The point of the tube at *h* is now unsealed or broken off, and the mercury issues drop by drop into a vessel placed to receive it.

The operation now proceeds continuously, and the apparatus requires scarcely any attention, further than to keep the cistern properly supplied with mercury, and to remove the pure metal when necessary. The residual air is quickly removed from the tubes by the pumping effect in *g*, and after a short time each drop falls with a sharp click in the tube. The construction of the part *h* makes it easy to obtain pure mercury from the very beginning of the operation, an advantage not furnished by the other forms of the apparatus mentioned.

In adjusting the height of the cistern, *a*, allowance must be made for the tension of the mercury vapor in the upper portion of the tube. The cooling effect of the condensing tube, *e*, is such that this is usually from four to six millimeters, and it rarely or never exceeds one centimeter. The temperature of vaporization corresponding to the latter tension is less than 180°, as, according to Regnault's results, this is the temperature at which the vapor has a tension of eleven millimeters. The low temperature is of itself a matter of importance, both as regards economy in the application of the heat, and as diminishing the probability of volatilization of any substances which the mercury may contain as impurities.

The apparatus here described, when in use, consumes from one-third to one-half the amount of gas required for an ordinary Bunsen burner. The mercury does not come into active ebullition, but vaporizes quietly and entirely without shocks. The rate of distillation varies of course with the heat applied, but is from four hundred to four hundred and fifty grams per hour. After the burner is once adjusted the apparatus requires no attention and may be left to itself for hours, care being taken that the cistern contains sufficient mercury. When out of use the tubes are left with the mercury in them, remaining thus exhausted and ready for use at any time.

As the mercury in the bulb and the tube *b* retains all the impurities left behind in the process of distillation, these may at length accumulate in such quantity as to interfere with the proper operation of the apparatus, and to necessitate their removal. This is not likely to occur for a long time unless the mercury used is excessively impure. But when the removal is indispensable it may be effected either by opening *f*, allowing the mercury to descend into the cistern and thus be withdrawn, then refilling and exhausting as at first; or more simply by lowering the cistern until the mercury sinks below the bulb, in which case all but the small portion contained in *b* will run out into the cistern and can be drawn off by a siphon or otherwise, care being observed that it is not carried so low as to allow of the admission of air at the bottom of the tube.

The apparatus in operation has proved entirely satisfactory

in every respect, and extended use of it in the laboratory has suggested no modification. As mounted upon its frame it has a height of about 125 centimeters, and the base covers a space forty-five centimeters long and thirty-three wide. It is so light that it may readily be lifted and carried with one hand. The glass work was very skillfully constructed, after the design of the writer, by Mr. W. Baetz, of 96 Fulton street, New York City.

Yale College, Nov. 14, 1881.

SCIENTIFIC INTELLIGENCE.

I. Physics and Astronomy.

1. *Dynamo-Electric Machines.*—Sir W. THOMSON concludes from a simple mathematical analysis of the currents in a dynamo-electric machine, giving a continuous current, that the formula $E = \sqrt{RR'}$ holds; in which E is the resistance of the exterior circuit and RR′ are the resistances of the field magnets and the revolving bobbins. If r represents the ratio of the total work to the lost work and $\varepsilon = \frac{R'}{R}$ the formula $r = 1 + 2\sqrt{\varepsilon}$ results. The case considered is that of a dynamo-electric machine provided with a shunt circuit.—*Comptes Rendus*, No. 12, September, 1881, p. 474. J. T.

2. *Rotation of plane of Polarization of Light by the Earth's Magnetism.*—M. HENRI BECQUEREL states as the result of his experiments that the rays D traversing horizontally a column of sulphide of carbon of 1^{m} in length in a direction parallel to the magnetic needle, undergo at the temperature of 0° C. under the influence of the earth's magnetism at Paris, a magnetic rotation of $0'{\cdot}8697$. The direction of this rotation is from right to left for an observer reclining horizontally with his head toward the north. This number constitutes a natural constant by which we can convert into absolute measure the determinations of the magnetic rotations of the plane of polarization of light, and by which we can express the intensity of a magnetic field in terms of the rotation to which it gives rise. In the C. G. S. system, the above result is expressed by $1{\cdot}31 \times 10^{-5}$ which denotes the magnetic rotation of the D lines in a magnetic field of strength unity, between two points at a distance of unity.—*Comptes Rendus*, No. 12, September, 1881, p. 481. J. T.

3. *The value of the Ohm.*—Lord RAYLEIGH and SCHUSTER have redetermined the ohm by means of the original apparatus used by the Committee of the British Association and have obtained the value $0{,}9893\ \frac{\text{earth quadrant}}{\text{sec.}}$.—*Proc. Roy. Soc.*, xcii, pp. 104–141, 1881. J. T.

4. *Ephemeris of the Satellites of Mars.*—The following tables give a portion of the ephemeris, calculated by Professor H. S. Pritchett, including opposition time and the time of nearest approach. Table I gives the times of east and west elongation for Deimos, that for Dec. 13 and the alternate below being *West*, and the others *East*; table II gives the times of west elongation for Phobos. The effect of aberration (not included) would make the satellites about five minutes late at each elongation.

I. Deimos.

Date.	Wash M. T.		Pos. Ang.	Dist.	Date.	Wash. M. T.		Pos. Ang.	Dist.	Date.	Wash. M. T.		Pos. Ang.	Dist.
Dec.	h.	m.	°	″	Dec.	h.	m.	°	″	Dec.	h.	m.	°	″
13	21	19	249·7	53·2	20	19	47	----	----	27	18	12	----	----
14	12	27	----	----	21	10	54	----	----	28	9	20	----	----
15	3	35	----	----	22	2	2	----	----	29	0	28	----	----
15	18	43	----	----	22	17	10	----	----	29	15	36	----	----
16	9	51	----	----	23	8	19	----	----	30	6	44	----	----
17	0	59	----	----	23	23	26	----	----	30	21	52	----	----
17	16	7	----	----	24	14	33	----	----	31	13	0	----	----
18	7	15	----	----	25	5	41	----	----	Jan.				
18	22	23	----	----	25	20	49	----	----	1	4	7	----	----
19	13	31	----	----	26	11	57	----	----	1	19	15	245·9	52·4
20	4	39	248·6	53·7	27	3	5	----	----					

II. Phobos.

Date.	Wash M. T.		Pos. Ang.	Dist.	Date	Wash. M. T.		Pos. Ang.	Dist.	Date.	Wash. M. T.		Pos. Ang.	Dist.
Dec.	h.	m.	°	″	Dec	h.	m.	°	″	Dec	h	m	°	″
13	5	31	----	----	19	22	12	----	----	26	14	53	----	----
13	13	10	----	----	20	5	51	248·5	21·5	26	22	32	----	----
13	20	49	249·7	21·3	20	13	30	----	----	27	6	11	----	----
14	3	28	----	----	20	21	9	----	----	27	13	50	----	----
14	12	7	----	----	21	4	48	----	----	27	21	29	----	----
14	19	46	----	----	21	12	27	----	----	28	5	8	----	----
15	3	25	----	----	21	20	6	----	----	28	12	47	----	----
15	11	4	----	----	22	3	46	----	----	28	20	26	----	----
15	18	43	----	----	22	11	25	----	----	29	4	5	----	----
16	2	23	----	----	22	19	4	----	----	29	11	44	----	----
16	10	2	----	----	23	2	43	----	----	29	19	23	----	----
16	17	41	----	----	23	10	22	----	----	30	3	3	----	----
17	1	20	----	----	23	18	1	----	----	30	10	42	----	----
17	8	59	----	----	24	1	40	----	----	30	18	21	----	----
17	16	38	----	----	24	9	19	----	----	31	2	0	----	----
18	0	17	----	----	24	16	58	----	----	31	9	39	----	----
18	7	56	----	----	25	0	37	----	----	31	17	18	----	----
18	15	36	----	----	25	8	16	----	----	Jan				
18	23	14	----	----	25	15	55	----	----	1	0	57	246·0	21·1
19	6	53	----	----	25	23	34	247·3	21·4					
19	14	33	----	----	26	7	13	----	----					

II. Geology and Natural History.

1. *Geological Survey of Pennsylvania.*—The following volumes have recently been issued at Harrisburg:

Report of Progress in Jefferson County (numbered H6), by W. G. Platt, 216 pp. 8vo, with a colored map of the county.

Third Report of Progress in the Laboratory of the Survey at Harrisburg (numbered M3), by A. S. McCreath. 126 pp. 8vo, with a map.

The Geology of Erie and Crawford Counties (numbered Q4), by I. C. White. 406 pp. 8vo. Includes a paper on the Discovery of the Preglacial Outlet of Lake Erie by J. W. Spencer, Ph.D., with two maps.

The Geology of Blair County (numbered T), by Franklin Platt. 312 pp. 8vo, with Atlas.

The volumes all bear evidence of good work, both in the scientific and practical direction.

Mr. McCreath's Laboratory Report contains numerous analyses of iron ores, coals and cokes, and limestones, with some of fire-clays. Many of the iron ores are from beds of limonite associated with Lower Silurian limestones. The limestone formation No. 2 (or the Calciferous and Chazy), wherever found in Pennsylvania, is stated to have associated with it more or less important deposits of this iron ore, some of them at the bottom, others at the middle, and others at the top beneath the Trenton limestone; and these beds have supplied the larger part of the stock to the furnaces along the Lehigh, Schuylkill and Susquehanna rivers, and the whole of it to the furnaces of Mountain Creek Valley, in Cumberland County, and some others. They occur at intervals in the Cumberland Valley, from the Lehigh River to Maryland, and through Virginia and East Tennessee to Alabama. Other iron ores analyzed were from *Magnetite* mines near Dillsbury, in York Co., connected with the *Mesozoic* sandstone, and still others from Devonian and Carboniferous rocks, and from bogs. The Cumberland Valley ores contain ·018 to 1·787 per cent of phosphorous, but usually under 0·5; and they sometimes vary in this respect 0·21 in the same bed.

In the Report on Erie and Crawford Counties Mr. White mentions facts respecting "buried valleys." He states that "the present water-courses meander along the upper surfaces of drift deposits which fill the ancient valleys to various heights above the old rock-beds." About four and a half miles below Meadville, in the valley of French Creek, a boring went down 285 feet through the drift from a level 482 feet above Lake Erie. Conneaut Creek has a drift-filling, according to borings, 180 feet deep. Other similar facts are reported. Conneaut Creek is the only one of the streams that now takes water to Lake Erie. The author refers to similar facts described in the Report of Mr. J. F. Carll, and cites his conclusion that the buried water-ways drained northwestern Pennsylvania toward Lake Erie. Mr. White states, as his own conclusion, that they owe their origin to glacial movement in the opposite direction. Mr. White's Report is occupied mainly with stratigraphical details, but treats also of the disturbances of the region, and of oil-wells and other points of general interest. The oil or petroleum is attributed to generation *in situ* from seaweeds, as urged by Lesquereux. He mentions the occurrence of a grit saturated with oil, in all parts of which were frag-

ments of trees, "like a fallen forest, or rather like a matted natural river-raft." A thin film of coal occurs on some specimens, "but in most cases the wood looks as if it had been converted into petroleum." In the underlying Venango Lower Sandstone and the Chemung flagstones no trace of oil was found, and "the horizontality, the absence of faults, slides, fissures or crushes of any kind, make the ascent of petroleum in the shape of gas a physical impossibility." The paper in this report by Dr. Spencer, on the preglacial outlet of the Lake Erie Basin (into Lake Ontario), has been noticed in this volume on p. 151. Professor Lesley accepts of the general conclusion, but with reference to the suggested origin of the lake-basins by the eroding action of a great ancient St. Lawrence River, he makes the modifying statement that the lake basins "although they may have been traversed by a great river were not properly excavated by it," but by the general abrading action of rills and streams from the rains descending the slopes into it, and probably by the removal of subjacent limestone beds by undermining erosion. To make the drainage system through the Great Lakes complete, so that the excavation by river action could be carried through to the sea, it is necessary to find an outlet for Lake Ontario cut down over 600 feet below the channel of the St. Lawrence, for the lake is over 700 feet deep; and on this point no facts or satisfactory suggestions are given.

2. *First Annual Report of the U. S. Geological Survey;* by CLARENCE KING, Director. 79 pp. roy. 8vo. Washington, 1880. —This volume (recently issued) contains, in reports from Mr. King and the several members of the Geological Survey, a brief review of the work done during the year ending June 30, 1880. The facts stated in these summaries are a promise of a very valuable series of reports on the several regions investigated; and the assurance is given on page 69 of the speedy completion of twelve volumes, as follows: *Geology and Mining Industry of Leadville*, by S. F. EMMONS; *Geology of Eureka Mining District, Nevada*, by A. HAGUE; *The Copper rocks of Lake Superior and their continuation through Minnesota*, by R. D. IRVING; *History of the Comstock mines*, by ELIOT LORD; *the Comstock Lode*, by G. F. BECKER; *Mechanical Appliances used in Mining and Milling on the Comstock Lode*, by W. R. ECKART; *Coal of the United States*, by R. PUMPELLY; *Iron in the United States*, by R. PUMPELLY; *the Precious Metals*, by CLARENCE KING; *Uinkaret Plateau*, by C. E. DUTTON; *Lake Bonneville*, by G. K. GILBERT; *Dinocerata*, by Professor O. C. MARSH.

3. *The Kames of Maine;* by G. H. STONE. 40 pp. 8vo. From the Proceedings of the Boston Society of Natural History, xx, 430–469.—The author describes "kames" as observed by him over a large part of the State of Maine, and on a map gives their positions. They include "kame ridges, and also terrace-like kame-plains." The kames sometimes follow valleys; "freely cross low transverse hills;" are seldom "deflected by hills less

than 100 feet high;" in "no instance cross any hill where, coming from the north, one would have to rise more than about 200 feet in crossing it; in fact the courses of the kames are curiously arbitrary." His theory of their origin is essentially that of Mr. Upham, cited on page 456.

4. *Geology of Staten Island.*—Mr. N. L. BRITTON has an article, in the School of Mines Quarterly (New York) for May last, on the geology of Staten Island—the large island lying to the south-southwest of New York Island. The geological map accompanying it represents the serpentine area as running nearly through the island, from New Brighton and Stapleton on the north (or rather from Constable Point just north of the island); gneiss as lying against this area on the east; Triassic sandstone and trap on the west; Cretaceous beds on the eastern and southern sides. A geological section is given; but as the gneiss outcrops only near Stapleton, and no strike or dip was taken, it is almost wholly ideal, and, considering the facts on New York Island, its details are very improbable. The asbestos exported from the island—which is only fibrous serpentine and contains therefore 12 to 14 per cent of water—comes from the area near Tompkinsville Landing.

Along with the serpentine or "steatitic rocks, occur superficial deposits of *limonite*, which have resulted from the decomposition of the rocks, in place." It is stated that the amount hitherto mined may be as great as 250,000 tons, while that now annually mined is about 20,000 tons. The limonite of the serpentine area of Rye, New York, (this Journal, II, xx, 32, 1880), is another example of the ore made from the iron minerals of a serpentine region; but at Rye there is some ferriferous dolomite with the serpentine, while the occurrence of disseminated limestone or dolomite on Staten Island is not mentioned.

5. *Apuan Alps.*—A paper on the geology of the Apuan Alps, by B. LOTTI and D. ZACCAGNA, is contained in the R. Comitato Geologico d' Italia, Bulletin Nos. 1 and 2, 1881. The rocks below the lias, are stated to include, beginning below—

a. The central schists: mica schist, talc schist, gneissic and argillaceous schist, with lenticular masses of calciferous schist containing *Orthoceras.*

b. The zone of the Grezzoni: the rock so called being a rough-looking impure limestone sparingly fossiliferous, subcrystalline or ceroid and brecciform; afforded De Stefani a fossil undoubtedly Triassic, *Turbo solitarius;* about five hundred meters on an average in thickness.

c. The zone of the marbles: saccharoidal limestone and dolomite, about 1,000 meters; some traces of *Crinoids* and *Chemnitzia.*

d. The zone of the superior schists: consisting of an alternation of schists, Cipolin marbles, calciferous, micaceous and arenaceous schists, with beds affording *Pentacrinus* and small ammonites of the genera *Phylloceras* and *Ægoceras;* 200 to 1,000 meters in thickness.

The paper gives detailed descriptions with sections.

6. *Jelly-like carbonaceous mineral resembling dopplerite, from a peat bed in Scranton, Pennsylvania.*—An article by Mr. T. COOPER in the number of the Engineering and Mining Journal for Aug. 13, contains the following interesting facts: The remarkable material was discovered in excavating for the new court-house of Scranton. This building-site is in the heart of the town, upon a square which formerly was a swamp, but some years ago was filled with cinder from the iron-works. On excavating for the court-house foundations, the cinder, which was five or six feet deep, was first removed. After this, came a bed of excellent peat, varying in depth from eight to twelve feet. Below the peat, a stratum of muck separated the peat from the hard-pan below. In the muck were veins of the tough black jelly, resembling coal in aspect, except its gelatinous character. When dried slowly it solidifies into a hard, brittle substance, which would be considered by an ordinary observer real anthracite coal. After hardening it does not again soften in water, hot or cold. It burns at a red heat, and leaves an ash resembling the red ash of some coals. It flames on first ignition. The jelly is acted on by alkaline solutions.

A letter to the editors, from Mr. H. Wright, secretary of the Wyoming Historical and Geological Society, dated Wilkesbarre, Aug. 27, 1881, states that an analysis made by the State Chemist afforded

Water, at 212° F.,	66·758
Volatile matter,	9 826
Fixed carbon,	4·012
Ash,	19·404
	100·000

7. *Emeralds from Alexander County, North Carolina.*—Mr. W. E. Hidden, whose important mineralogical labors in North Carolina have been previously mentioned in this Journal (xx, 150; xxi, 128, 159, 160; xxii, 21, 179), has recently announced the discovery by him of emeralds sixteen miles northwest of Statesville in Alexander County, North Carolina. The occurrence of beryls of unusual beauty and crystallographic interest was made known some years since by Mr. J. Adlai Stephenson. Mr. Hidden was led by this fact to make thorough and systematic search in the hope of finding them in place, and he has succeeded in finding not only the ordinary beryls but also true emeralds. The prevailing rock of the region is a feldspathic gneiss with a strike N.N.W., and nearly vertical dip. The surface soil often contains crystals of quartz, rutile, tourmaline, spodumene, beryl, etc., and in cross-fractures in the rock beneath, the minerals have been found by Mr. Hidden in place; of these minerals the emerald-green spodumene (*hiddenite*), and the true emeralds have been the special objects of search because of their value as gems. The first pocket found has been worked to a depth of thirty-three feet and has yielded largely of spodumene, but sparingly of the emeralds; twelve similar cavities have been found within an area of

forty feet square yielding emeralds, while still others have afforded quartz, rutile, monazite, mica and other species. So far as the explorations have been carried, the pockets have been in a crumbling condition and the crystals have been found detached, lying in the bottom of the cavities. As the work is carried down deeper it is to be expected that the rock will increase in firmness. The largest cavity yet discovered had a depth of sixteen feet, and was three feet wide and seven in length. The surface walls were thickly studded with large crystals of quartz, some of twenty-five pounds in weight, and with them nine fine emeralds. Their form was that of a twelve-sided prism (*I* and *i*-2), with basal planes, all well polished. The largest crystal had a length of eight and one-half inches and an average diameter of one inch. The others varied in length from two to six inches. Most of the crystals found are vertically deeply striated or ribbed, and are transparent, though not free from flaws. In some of the crystals the color near the surface is the deepest and the core is nearly colorless. The North Carolina emeralds do not quite equal in color those from Muso, New Granada, but are nevertheless very beautiful and will bear comparison with those from other known localities.

8. ***Brief notices of some recently described minerals.*** (Continued from page 155.) ILESITE.—A white friable mineral with a bitter, astringent taste, readily soluble in cold water. An analysis afforded Dr. Iles—SO_3 35·85, MnO 23·18, FeO 4·55, ZnO 5·63 H_2O 30·18=99·39, corresponding approximately to Mn(Fe, Zn) SO_4+4aq. Occurs with pyrite and sphalerite forming a band two to eight inches in width; locality, Hall Valley, Park Co., Colorado. Named after Dr. M. W. Iles, of Leadville.—*Mining Index*, Leadville, Nov. 5, 1881.

SEMSEYITE.—Briefly mentioned by Kreuner as a mineral containing lead, antimony and sulphur, occurring in gray crystals, and resembling plagionite. Found with diaphorite, sphalerite and pyrite at Felsöbanya.—*Ungarische Revue*, April, 1881.

ÅNNERÖDITE.—Occurs in crystals closely related to columbite both in habit and angles. H.=6. G.=5·7. Luster metallic to submetallic. Color, black to blackish-brown. Translucent in thin splinters. Fracture sub-conchoidal. An analysis by C. W. Blomstrand gave Cb_2O_5 48·13, SnO_2 0·16, SiO_2 2·51, ZrO_2 1·97, ThO_2 2·37, U_2O_3 16·28, Ce_2O_3 2·56, Y_2O_3 7·10, PbO 2·40, FeO 3·38, MnO 0·20, CaO 3·35, MgO 0·15, K_2O 0·16, Na_2O 0·32, Al_2O_3 0·28, H_2O 8·19=99.51. The formula deduced is $R_2Cb_2O_7+2\frac{1}{2}$ aq, which makes the mineral related in composition to samarskite. Found in a pegmatite vein at Ånnerod, near Moss, Norway. Described by W. C. Brögger.—*Geol. För. i. Stockholm Förhandl.*, v, 354, 1881.

ZINCALLUMINITE.—Found in very small thin hexagonal crystals; optically, uniaxial negative. Color white, or slightly tinted with blue. An analysis by Damour gave SO_3 12·94, Al_2O_3 25·48, ZnO 34·69, CuO 1·85, H_2O 25·04=100. From the

zinc mines at Laurium, Greece, associated with smithsonite, serpierite and several undetermined species. Described by Bertrand and Damour.—*Bull. Soc. Min. de France*, iv, 135, 136, 1881.

ALASKAITE.—Massive, small foliated. G.=6·878. Luster, metallic. Color, whitish lead-gray. Opaque. Analysis (after deducting impurities), S 17·63, Bi 56·97, Sb 0·62, Pb 11·79, Ag 8·74, Cu 3·46, Zn 0·79=100; another analysis gave 3 p. c. Ag, and 5·38 p. c. Cu. The formula deduced is $(R,\dot{R})S+Bi_2S_3$. Occurs intimately mixed with quartz, barite, chalcopyrite and tetrahedrite at the Alaska mine, Poughkeepsie Gulch, Colorado. Described by G. A. König.—*Amer. Phil. Soc. Philad.*, 1881, 472.

9. *Artificial formation of the Potash-feldspar, Orthoclase;* by C. FRIEDEL and E. SARASIN (Bull. Soc. Min. de France, iv, 171). —The process used by these chemists for the formation of orthoclase in crystals consisted in heating together in a tube of steel having red copper within, for 15 to 20 hours to a temperature between 400 and 500° C., a mixture one part of aluminum silicate and another of a potassium silicate rich in alkali. A higher temperature was disadvantageous, it producing a crystallization of the silica either as quartz or as tridymite. The trials gave a crystalline powder, which was made up of crystals of orthoclase large enough to be studied crystallographically. Thoulet's method gave for the specific gravity that of orthoclase. An analysis afforded alumina 15·59, potash 14·38, leaving for the silica 70·03. There is here an excess of silica of 6·30 per cent, which was due to the presence of some free silica; the other ingredients have the orthoclase proportions. The authors did not succeed when the mixture was made to consist of silica, alumina and potash, in the proportions they have in orthoclase.

10. *English Plant-Names from the Tenth to the Fifteenth Century.* By JOHN EARLE, M.A., Rector of Swanswick, Professor of Anglo-Saxon in University of Oxford. Oxford: Clarendon Press, 1880. 16mo, pp. cxii and 122.—A notable little book, consisting in the first place,—yet in the volume occupying the last place,—of sundry Saxon vocabularies in which "the native plant-names have been preserved in the most primitive form extant, printed for the use of friends of Saxon studies" without any idea of making a book. To this is prefixed an Introduction, on the history of plant-names from Theophrastus down to the modern system of nomenclature; the signification of the old native plant-names; their relation to the Roman ones; grammatical elements of English plant-names; on the neglect of vernacular names, etc. Of the matters linguistic we are not now to speak; and probably Professor Earle is only a superficial botanist. But his sketch of the history of nomenclature, and of the development of mere herb-lore or the rude knowledge of simples into botanical science is as critically excellent as it is terse and fresh. Indeed, we know of nothing half so good within so small a compass. Then we begin to understand "the fascination of vernacular plant-names," which, as the author remarks, "has its foundation in two instincts,

the love of nature and curiosity about language. Plant-names are often of the highest antiquity and more or less common to the whole stream of related nations. Could we penetrate to the original suggestive idea that called forth the name, it would bring valuable information about the first openings of the human mind towards Nature; and the merest dream of such a discovery invests with a strange charm the words that could tell, if we could understand, so much of the forgotten infancy of the human race."

Here is a good word for the amiable science, considered educationally. "Historically almost the first of sciences, Botany is naturally and eductionally [educationally?] first in order to the enquiring mind. Its objects are near our homes, awakening to our minds, and inviting to our touch. Botany is adapted to be the universal preparatory science, the science to infuse the scientific sense."

While giving a series of examples of the changing meanings of a certain class of words, the author goes singularly astray in a single instance: e. g. "In England *farmer* means an occupier, in America it means a hired labourer." No, indeed: it means a cultivator of the land who is *not* a hired laborer: he is commonly the owner of his *farm* in fee simple. A. G.

11. *Familien Podostemaceæ.* Studier af Dr. EUG. WARMING. 1te Afhandlung.—This is a paper in the Memoirs of the Royal Academy of Sciences of Copenhagen, being the commencement or first part of an extended treatise on the *Podostemaceæ*, morphological, anatomical, and systematic. This singular family of Phænogamous plants, simulating *Algæ* in vegetation, takes its name from our *Podostemon ceratophyllus*, of Michaux's Flora, the only North American representative, and the only one inhabiting the North temperate zone. Having been well supplied by Mr. Canby with a stock of plants in spirit, in all stages of growth, Dr. Warming has taken this species for particular study, and his anatomical and morphological investigation of its organs of vegetation is here presented. The body of the article is in the Danish language. But an abstract and also the full explanation of the plates are in French. The whole fills 34 quarto pages and is illustrated by six plates, crowded with figures, drawn and lithographed by the author himself. Three of the six plates and half of the fourth are devoted to our *Podostemon.* A. G.

12. *Recherches sur la physiologie et la morphologie des ferments alcooliques.* By EMIL CHR. HANSEN.—The present paper, extracted from the proceedings of the physiological laboratory of Carlsberg, Copenhagen, for 1881, treats of *Saccharomyces apiculatus* and its occurrence in nature. This ferment, according to Hansen, is found during the warm season on juicy fruits, as gooseberries, cherries, plums, etc., and is carried to the earth by winds and rain and passes the winter buried in the soil. In fermentation it acts as a bottom yeast but possesses only a feeble action, since, while the common yeast, *Sacc. cerevisiæ* produces six volumes of alcohol, *Sacc. apiculatus*, produces only one. The beer

which it produces has a peculiar taste and odor. The species produces no invertine, nor can it cause an alcoholic fermentation in saccharose solutions. The cells are very tenacious of life, can be kept dried several months, and in this condition exposed to marked variations of the thermometer without apparent injury.

W. G. F.

13. *On an Organism which penetrates and excavates Siliceous Sponge-spicules* (Spongiophagus Carteri); by Professor P. MARTIN DUNCAN.—In a communication which I made to the Royal Microscopical Society on June 8, 1881, the presence of green-colored cells on siliceous sponge-spicula, in relation to minute penetrations into their axial canals, was asserted. The occurrence of a granular plasma of the same tint within enlargements of the axial canals was noticed; and the penetration and erosion were stated to be due to the organism. The cells which were observed within hollows on the surface of a spicule, and also on perfect spicules in positions where erosion from without inwards could readily occur, were very small,—not more than $\frac{1}{7000}$ inch in length, and very much less in height. Their dimensions, however, corresponded to those of certain circular patches with hollowed-out bases, which are the first stages of the penetration through the spicule down to the axial canal. The penetration of the spicule down to the central canal is followed by the growth of the organism, which appears to erode the silica and enlarges the canal in a most remarkable manner.

After a while the spicule suffers solution of its continuity by the thinning from within, and the thinnest flakes present a granulated appearance.

Since writing that communication I have observed siliceous sponge-spicules, obtained from great depths, which are affected by an organism whose cells are much larger and whose penetrations therefore are wider and much more visible. On the head of a large spinulate spicule I found many circular pits, each containing an organic mass without definite cell-wall, and yet granular and green in color by transmitted light. These pits are shallow and are $\frac{1}{2000}$ inch in diameter. Similar pits and of the same dimensions are seen on other spicules; but they are deep and resemble cylindrical tubes with hollowed-out bottoms. Some reach the axial canal, which has become enlarged. The penetrations contain granular organic substance; and so do the enlarged axial canals. The walls of the enlarged axial canals are frequently very irregularly eroded and look "worm-eaten;" the hollows are, moreover, green with the very visible granular matter.

Thus there are two dimensions of the penetrations. The first kind of cell found on the spicules resembles somewhat the simple zoospores of *Achlya penetrans* Duncan (Proc. Royal Soc., vol xxv, pl. vi); the second is larger; and in both there is a decided green tint. No ramifications of the penetrating cylindrical tube occur; and it pierces perpendicularly to the surface of the spicule, or, it may be, slightly aslant.

The presence of pits on the surface of sponge-spicules was noticed by Kölliker as a peculiar degeneration of the structure. Dr. Carter described and figured pits in the outer part of a spicule, and distinctly referred them to the action of a vegetable cell, in the Ann. & Mag. Nat. Hist. ser. 4, vol. xii, p. 457, pl. xvi, figs. 8, 9. None of the pits seen by my friend reaches the axial canal; but some of them terminate in globular excavations.

It is evident that the assimilation of the organic substance in the sponge-spicule by the vegetable organism produces the destruction of the siliceous structure; and probably the colloid silica unites with the protoplasm of the destroyer and forms an organic compound with it.

Large cells and small nucleus-like cells operate, producing penetrations of corresponding diameters through the spicule down to the axial canal. The vegetable growth occurs there; and the amount of erosion does not appear to be in relation with the size of the primary penetration.

The organism is not an *Achlya;* and all that can be said is that it consists of cell-like bodies without very definite cell-walls, but evidently with a very delicately limiting texture surrounding a granular greenish plasma, and that there is much free and non-cellular plasma with bodies like small nuclei, the whole having a faint green tint. I have named this very lowly organic substance (which is probably a plant) *Spongiophagus Carteri.—Ann. & Mag. Nat. Hist.*, Aug., 1881, p. 120.

14. *Bulletin of the Museum of Comparative Zoology at Harvard College.* Vol. VI, Part ii, No. 12. E. L. Mark on the *Maturation, Fecundation and Segmentation of Limax campestris* Binney. pp. 173–625, 8vo, with 5 double plates.—A profound microscopic research throwing new light on the metamorphosis of the nucleus and other points in the earliest stages of egg-development, reviewing at length, with criticisms, previous researches on the subject, and giving an extended bibliography.

15. *The Palæocrinoidea.*—Part II of Wachsmuth and Springer's revision of the Palæocrinoidea is contained in the Proceedings of the Academy of Natural Sciences of Philadelphia for 1881, commencing with page 177. It is devoted to the Family Sphæroidocrinidæ, under which are included the Sub-families Platycrinidæ, Rhodocrinidæ and Actinocrinidæ. It is a long and very valuable paper.

Cosmos les Mondes: Revue hebdomadaire des Sciences et de l'Industrie, fondée et dirigée pars M. l'Abbé F. Moigno, Paris.—The valuable weekly review, *Les Mondes*, commenced by M. l'Abbé Moigno in 1852, appears now in new form, enlarged in size and improved in appearance. The Abbé still retains the direction of the review, but he has the assistance of a group of collaborators, under whose combined efforts it promises to have an increased sphere of usefulness in the future.

INDEX TO VOLUME XXII.*

* This Index contains the general heads BOTANY, GEOLOGY, MINERALOGY, OBITUARY, ZOOLOGY, and under each the titles of Articles referring thereto are mentioned.

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